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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND THE ARTS:

*ILLUSTRATED WITH ENGRAVINGS.*

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BY WILLIAM NICHOLSON.

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V O L. IV.

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L O N D O N:

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# JOURNAL



NATURAL PHILOSOPHY, CHEMISTRY,

AND THE ARTS:

ILLUSTRATED WITH ENGRAVINGS

BY WILLIAM NICHOLSON

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The mercurial air pump consists of two vertical pipes containing mercury, in one of which is a solid piston, and at the extremity of the other is a valve opening upwards. The mercury in effect answers the purpose of a piston, fitted with a degree of accuracy not to be expected in solid work. The double barreled pump has the contrivance of a hole instead of a lower valve. Its pistons have valves, but by the very happy disposition of a cock, the effect of the apparatus may be so changed that, instead of both barrels working from the receiver, the one is made to clear the other very powerfully from the residual air. Observations to shew certain imperfections, and the inefficacy of the lower valve in Cuthbertson's Air Pump.

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- II. Description of an Engine for raising Water by the lateral Motion of a Stream of Water through a conical Tube. In two Letters from Mr. William Close - - - p. 293

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I. Notice of some Observations on the Causes of the Galvanic Phenomena, and on certain Modes of increasing the Powers of the Galvanic Pile of Volta. By Mr. Davy, Superintendent of the Pneumatic Institution. Communicated by the Author p. 337

Zinc in the Galvanic circuit undergoes no oxidation at common temperatures with pure water; but it is oxidized when the water contains either atmospheric air, oxygen, nitrous gas, nitrous acid, or marine acid, &c. These substances at the same time undergo chemical change. The pile of Volta does not act with pure water; but it does when the water contains any of the last enumerated substances. Its power appears to be proportional to the oxidation of the zinc; which may therefore be esteemed the cause. This conclusion leads to methods of constructing piles of immense power. Experiments in proof. Pile with nitrous acid at least six times as strong as the common pile.

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V. Experiments on the solar and on the terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same or different. By William Herschel, LL. D. F. R. S. p. 360

A series of experiments were made in which the light of the sun, of a candle, of the solar prismatic spectrum, of a red hot poker, and of a coal fire, were severally reflected from metallic mirrors, and produced heat. The invisible radiance from the same objects was also made to undergo optical reflection and condensation, and produced heat. Experiments of the same kind were made by diverting the visible as well as invisible radiance, of the same objects out of its course by refraction, and condensing it by the same means, in all which cases heat was produced.

VI. On the Arragonite of Werner. By Cit. Haüy p. 367

VII. On the true Origin of Resin, known by the Name of *Sandarac*, and on that of Gum Arabic, by M. Schouboe p. 369

VIII. Remarks on the Memoir in which M. Girtanner examines whether Azote be a simple or compound Body. By Cit. Berthollet p. 371

IX. On



IX. On the Decomposition of the Muriatic Acid. By Mr. John Pitchford, Junior - - - - - p. 374

X. Observations on the Action which the Sulphate of Iron exercises upon Nitrous gas, and on the Formation of Muriatic Acid. By Cit. Berthollet - - - - - p. 377

Scientific News, Accounts of Books, &c. - - - - - p. 379—384

Philosophical Transactions of the Royal Society of London, for the Year 1800. Part the Second. Fossil Wood, found at a very great Elevation.---Extract of a Letter from Mr. Humphry Davy, dated October 23. Supplementary to his Paper on Galvanism, in the present Number.---Analysis of various Fossils.---Lime in the Nut Gall.---Extraction of the Alkali of Sea Salt by Lime. Method of obtaining Phosphorus by Phosphate of Lead.---On the Mineral Kermes, and the golden Sulphur of Antimony.---Elastic Resin in Opium.---Discovery of a New Earth in the Beryl of Georgen-Stadt.---On the Method of hearing by the Teeth.

## DECEMBER 1800.

Engravings of the following Objects: 1. Construction of a Wheel to represent the Planetary Motions. And, 2. Method of graduating the Hydrometer, so as to indicate the Strength of Spirit by Inspection at any Temperature.

I. On the Number of the Primitive Colorific Rays in Solar Light. By the Rev. Matthew Young, D.D. S.F.T.C.D. and M.R.I.A. - - - - - p. 385

Whether the primitive colours be composed of three original tints or more? cannot be determined by mixture of powders, &c. That light of one colour is not all of the same refrangibility, is inferred from the extent of each colour on the spectrum. Probability that red, yellow and blue are the primitive colours, because any two of these adjacent to each other will compose the intermediate tinge; though these supposed originals cannot be so composed of the others, Theoretic observations, experimental test of this doctrine by coloured glasses. Ocular spectra. Hypothesis of three colours will solve the phenomena of chromatics.

II. An Account of some Additional Experiments and Observations on the Galvanic Phenomena. By Mr. Davy, Superintendant of the Pneumatic Institution. Communicated by the Author - - - - - p. 394

Sulphuric acid excites the pile very little, but becomes active by the addition of water. Liquid sulphurets are equal in power to water. Nitric and sulphuric acids act in vacuo. Observations, &c. Enquiries to determine what happens in the pile itself. Examination of the power of different Galvanic combinations.

III. On the Quotients arising from the Division of an Unit by prime Numbers. By H. Goodwyn, Esq. - - - - - p. 402

IV. Construction of a Wheel adapted to express, by its Rotation, the unequal angular Motion of the Planets. By M. Roemer - - - - - p. 404

V. On

- V. On the Solutions and Precipitates of Mercury. By Cit. Berthollet p. 405
- VI. A Chemical Analysis of Three Species of Whinstone, and Two of Lava. By Robert Kennedy, M.D. F.R.S. & F.A.S. Edin. p. 407
- VII. Further Remarks on the Enquiries of Dr. Herschel, respecting Light and Heat. In a Letter from Mr. John Leslie - p. 416
- VIII. Experiments and Observations on the Light which is spontaneously emitted, with some Degree of Permanency, from various Bodies. By Nathaniel Hulme, M.D. F.R.S. and A.S. - - - p. 421
- IX. On Areometry; more particularly as it relates to Alcohol of different Strengths and Temperatures. By Cit. Hassenfratz - - - p. 428

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J A N U A R Y 1801.

- Engravings of the following Objects: 1. Mr. Edgeworth's Panorganon, or Apparatus for teaching the first Principles of Mechanics. And, 2. A new rotatory Engine for raising Water.
- I. Construction and Use of an universal Table of Interest. By H. Goodwyn, Esq. - - - - - p. 433
- II. A Chemical Analysis of Three Species of Whinstone, and Two of Lava. By Robert Kennedy, M.D. F.R.S. & F.A.S. Edin. (Concluded from p. 415.) p. 438
- III. Description and Use of a cheap and simple Apparatus for teaching the first Principles of Mechanics. By Richard Lovell Edgeworth, Esq. F.R.S. and M.R.I.A. - - - - - p. 443
- IV. Experiments and Observations on the Light which is spontaneously emitted, with some Degree of Permanency, from various Bodies. By Nathaniel Hulme, M.D. F.R.S. and A.S. (Concluded from page 427) p. 451
- V. Observations on the Theory of Electric Attraction and Repulsion. By the Rev. George Miller, F.T.C.D. - - - - - p. 461
- VI. Description of a new rotatory Engine for raising water, and for other Purposes. By a Correspondent - - - - - p. 466
- VII. Account of a new Method of bleaching Cotton, as published by Chaptal, Member of the National Institute. By J. C. Delametherie p. 469
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VIII. On the Chemical Effects of the Pile of Volta. By a Correspondent  
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IX. Memoir of several newly discovered Properties of phosphorated Hydrogen Gas. By Citizen Raymond, Professor of Chemistry at the Central School of the Department of the Ardèche  
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Scientific News, Accounts of Books, &c.  
p. 475—480

Premiums offered by the Board of Agriculture.---Prizes offered by the Class of Mathematical and Physical Sciences of the National Institute of France in its Public Sitting, 15th Germinal in the year 8 (April 4, 1800.)---Candles with wooden Wicks.---Ink capable of resisting the Action of Oxygenated Muriatic Acid.

## F E B R U A R Y 1801.

Engravings of the following Objects: 1. An improved Hydraulic Engine, by Mr. Clofe. 2. The Duke of Bridgewater's underground inclined Plane. 3. Figures illustrative of Galvanic Discoveries. And, 4 Mr. Home's Exhibition of the Structure of that singular Animal the Ornithorhynchus paradoxus.

I. Observations on a Method of restoring the Utility of Wells, which have been abandoned in consequence of the Mephitization of the Ground. By Cit. Cadet-de-Vaux, of the Society of Agriculture of the Department of the Seine in France) Communicated at the Sitting of that Society on the 16 Brumaire, in the Year 8  
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The method consists in boring and then driving a wooden pipe into the cavity. Through this pipe the borer is made to act and increase the depth. Another pipe is driven in so as to sink the first still lower. By a continuation of this process the length of pipe is carried to very great depths, if necessary, and water is conducted from the lower springs to the surface.

II. Memoir of several newly discovered Properties of Phosphorated Hydrogen Gas. By Citizen Raymond, Professor of Chemistry at the Central School of the Department of the Ardèche. (Concluded from page 475)  
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III. Description of the underground inclined Plane, executed at Walkden Moor in Lancashire, by His Grace the Duke of Bridgewater. By the Rev. Francis Egerton  
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- VI. Some Observations on the Head of the *Ornithorhynchus paradoxus*. By Everard Home, Esq. F. R. S. p. 506
- VII. Instances of suspended Animation in Vegetables. By Mr. John Gough p. 509
- VIII. Extract of a Letter from Doctor G. M. to Dr. William Babington, dated Freiberg, Dec. 17, 1800. On the State of Galvanism and other scientific Pursuits in Germany. Communicated by Dr. Babington p. 511
- IX. On the Chemical Effects of the Pile of Volta. By a Correspondent p. 514
- X. Analysis of the Mellite, or in German, Honigstein. By Citizen Vauquelin p. 515
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Method of ascertaining the Inclination of the Magnetic Needle. By Cit. Coulomb.---New Production of Ammonia.---Discovery of a new Alkali.---Flour from the Bread Fruit.---Extract from a Memoir of Cit. Thénard, on the several Degrees of Oxigenation of the Oxide of Antimony, and on its Combinations with sulphurated Hydrogen.---On the Structure of the Upper Pyreneans, by Cit. Raymond. Communicated to the National Institute of France.---Letter from Mr. Davy, Superintendent of the Pneumatic Institution, containing Notices concerning Galvanism. Parkinson's Chemical Pocket Book.---Mr. Henry's Pocket Volume on Chemistry.

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## M A R C H 1801.

- Engravings of the following Objects: 1. A very simple Steam Engine, by Mr. Nancarrow. 2. Apparatus for Experiments with Heat, by Mr. Thompson. 3. New application of the Syphon, by Mr. Close; and, 4. Figure of the *Ornithorhynchus paradoxus*.
- I. Experiments to determine whether or not Fluids be Conductors of Caloric. By Thomas Thomson, M.D. Lecturer on Chemistry in Edinburgh. Communicated by the Author p. 529
- II. Description of a Steam Engine on the Principle of Savary, operating by a separate condenser; with other essential Improvements. By Mr. John Nancarrow p. 545
- III. New Application of the Syphon to raise Water above the surface of the Reservoir. In a Letter from Mr. William Close p. 547
- IV. On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions, illustrated by select Observations. By William Herschel, LL. D. F. R. S. (Continued from page 506.) p. 550
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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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APRIL 1800.

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ARTICLE I.

*On the Antiquity of the Art of Etching on Glafs. By Mr. FRED. ACCUM.*

LOOKING over a parcel of old foreign newspapers, I was not a little surpris'd to observe that the art of etching on glafs had been known and practis'd amongst our ancestors upwards of a century ago.

In order that every one may judge for himself, whether this discovery belongs exclusively to Scheele, the Swedish chemist, I shall transcribe an advertisement inserted in an old German periodical publication\*, which is likewise copied into Beckman's Collections towards a History of Inventions. The original advertisement, literally translated, runs thus: "*The discovery of a powerful acid, by means of which every imaginable kind of figures may easily be etched upon glafs.*"

"Take spiritus nitri per destillationem, put it into a retort, and apply a strong heat. When it has passed over into the receiver, throw into it some powdered green Bohemian emerald (which, when heated, shines in the dark), otherwise called *hesphorus*. This being done, place the receiver, containing the mixture, on a heated sand-bath, for about four-and-twenty hours, and it will be fit for the purpose. To use this corrosive acid,

\* Breslauer's Collections for the year 1725: January. This receipt is likewise inserted by Krunitz in his Economical Encyclopedia, XI. p. 678.

*On the Antiquity of Etching on Glass.*

“ take a pane of glass of any kind, clean it well, and free it from grease, by washing with a ley, and when dry, trace out upon it, with sulphur and varnish, whatever you choose. Put a border of bees'-wax round it, about one inch high, and then pour the corrosive acid, prepared as before directed, carefully over the whole surface of the glass, and let it stand undisturbed for some time, the longer the better. The glass will become corroded, and all you had traced before, will now appear as if raised, or elevated, above the surface of the glass, in a very distinct and pleasing manner.”

This advertisement is signed *I. G. Wigand*, and dated from *Goldingen*, in Courland. In a subjoined note, the author of it confesses that he is not the inventor of the corrosive acid, but that he had the receipt from *M. Pauli*, a famous physician, resident at Dresden, and lately deceased. He further asserts, that the late doctor, during his life-time, had etched on glass several curious crests, names, and figures, in a manner no less beautiful than surprising.

In a supplement to the before mentioned publication, the same author very earnestly endeavours to convince the public, that the art of etching on glass he had made published some time before, was very different from the art of engraving on glass by means of emery, flint, red-hot iron, or other sharp pointed instruments of the hardest kind. Nor is it that art, says he, by which *Mathesius*, the famous apothecary, operated, who formed elevated, or engraved figures, festoons, and other ornaments on glasses, drinking cups, &c. for all this I know very well is done by means of diamond pencils. But the method I have made public, seems to be, in fact, the very same admirable and secret art which was formerly practised by a skilful artist of Nuremberg, who worked in that art solely for the emperor Charles II.

On pursuing this subject a little further, there remains, no doubt, but that this old artist of Nuremberg, to whom our author alludes, is the person of whom *Doppelmair* \* gives the following account: “ Amongst the artists of Nuremberg must be enumerated *Henry Swanhard*, a skilful engraver in steel and stone, who found out by the glass of his spectacles, upon which some corrosive liquid had fallen, a new art of etching upon glass, which discovery he made in the year 1670.” *Wagenfeil* †, on the contrary, relates, that the inventor derives more merit from his discovery, than accident entitled him to claim. “ *Swanhard*,” says he, “ by the acuteness of his genius, proved what was hitherto deemed impossible. He was the first who ever composed a corrosive, so active as to dissolve the hardest crystal glass, which hitherto resisted the force of the most powerful spirits of the apothecaries. By means of this corrosive, *Swanhard* delineated and etched on glass figures of men, some naked and some dressed, and all kinds of animals, flowers, plants, trees, and shrubs, in a manner perfectly natural, and perfectly easy.” The same is mentioned in *Schneider's Biography of the Artists of Germany*, 1740, p. 37.

How the above artist prepared his corrosive is not mentioned by any of the above authors. It is, therefore, very probable, that he kept his art a secret, as it seems that the

\* *Doppelmair's History of the Artists of Nuremberg*, 1734, p. 16.

† *Wagenfeil's Comment. de Civitate Norimbergensi*, Altdorf, 1697. 4to. p. 154.



receipt for that purpose was made public, for the first time; in January, 1725\*, by the before mentioned M. Pauli. How he came to know it, I have not been able to learn.

That the inventor employed his corrosive to a purpose different from that for which it is used at present, is obvious from the effect it produced upon the glass. At present the glass is covered either with wax, isinglass, or a varnish, and those figures which are intended to be etched, are traced through this covering, by means of a pointed instrument; but our ancient artist covered his figures, when traced, with a varnish and sulphur, and then applied the acid to corrode the glass around them, so that the figures which remained smooth and clear appeared, when the varnish was removed, as if raised from a dim or dark ground. He adopted this method probably in order to render his invention more palpably different from the art of cutting the figures on the glass, as if engraven, long known and practised before this discovery was made.

That the Bohemian emerald, or hesphorus, made use of in this receipt, was our fluat of lime, can hardly be doubted. In old German authors who treat on the working of mines, this stone is classed amongst the flusse (fluxes) because it was an usual substitute to accelerate the fusion of certain ores. *Agricola*, who changed the German names of quartz, zinc, bismuth, cobalt, &c. into quartzum, zincum, &c. changed fluss into fluor, which became afterwards very common †. If a passage of the ancients can be quoted that seems to allude to fluat of lime, it is that of Theophrastus, where he says, that there are certain stones which, when added to silver, copper, and iron ores, become fluid. For Cronstadt was the first systematic writer (if I am not mistaken) who mentioned fluat of lime as a particular genus.

That the stone called emerald, or hesphorus, was really our fluor spar, becomes obvious from what Kirchmair, professor at Wittemberg, in Germany, remarks, who was probably the first who made known what kind of stone it was, which had the peculiar property of shining in the dark after having been heated ‡; and which by the ancients was sought for

\* Bresslawer's Collections, l. c.

† Lapidés sunt gemmarum similes, sed minus duri, fluores liceat mihi verbum an e verbo exprimere, nostri metallici appellant, nec meo judicio, inepte; siquidem ignis calore, ut glacies solis, liquecunt et fluunt. Varii autem et jucundi colores eis insident.

BERGMANNII OPUSCUL, p. 466.

‡ Ante annos paucos admodum inventus mineralis alicujus visu et proprietatibus in quibusdam similis smaragdo, ab artificibus duobus mihi per amicè notis usus est. Conficiendi phosphori et repræsentandi modus levis atque brevis ille. Recipe q. v. mineralis viridis smaragdum pene referentis; contare in pulverem, aqua madefac communi, pulvis instar ut fiat. Pencillo postea in lamina cuprea, magnitudine vel majoris plani alicujus manubrio instructa literas, quascunque voles, in lamina describe crassiusculas. Ardentibus super impone prunis vasculo exceptis. Phænomenon spectabis in obscuro amænissimum sine fume et odore lucens. Sed ut verum fatear, nec usum, nisi curiosi animi explendi, artificium hoc, neque diuturnitatem habet. Peccus ergo avidum sciendi meliora fatiari nequit sive Hesperus vocetur sive Vesperugo.

G. C. Kirchmaieri De Phosphoris et natura lucis, nec non de igne, commentatio epistolica. Wittembergæ, 1680, p. 7.

with so much avidity. Neither need I to mention that the nitrous acid made use of in the receipt was equally capable of disengaging the fluoric acid, though we usually make use of the sulphuric.

There remains, therefore, little doubt, that the art of etching on glass does not entirely belong to the moderns, or to deny that the ancients were altogether unacquainted with it, would be doing them an injustice. It seems rather, that this art belongs to the discoveries which were made in those times, in which men were little inclined to transmit an account of their inventions to posterity, and thus this art must have been forgotten or lost. Scheele\* discovered the fluoric acid, and re-invented the art of etching on glass in the year 1771. But had Swanhard been able to pursue properly what either accident, or ingenuity presented to him, he might have enriched the arts, with a discovery which acquired great reputation to this Swedish philosopher one hundred years after.

FREDERIC ACCUM.

## II.

*A Description of a new Instrument called the Blast Ventilator. Invented by J. W. BOSWELL.*

IT would be superfluous to offer any arguments to the cultivators of natural philosophy, respecting the importance of any discovery in that science. It is sufficient to announce it to those who so well know, that no operation of nature is unimportant; and that although the first person to whom a fact may occur, may not be able to point out all the advantages which may arise from it, yet others of more discernment coming after him, may improve upon his ideas, and perhaps use it for purposes of much higher importance.

What Dr. Hales has written of the great consequence of the subject of ventilation to health, and the preservation of the food of man, renders it unnecessary to expatiate on the importance of any new discovery in this art; and to his excellent treatise on this matter, I beg leave to refer those who have any doubts on the advantages arising from it.

The method of forcing air in any required direction, by a fall of water passing from a smaller into a larger tube, in an engine called the † Water Blast, is now well known, and frequently applied in mines and furnaces with great efficacy.

Some considerations on this machine led me to conjecture long since, that a current of air might be made to act instead of a fall of water, in an apparatus constructed on principles somewhat similar to the above, and thus applied so as to draw off foul air or smoke from any places required, in situations where the other could not be used. It was only lately that I have been able to think of any method of putting this matter to the test of ex-

\* Transactions of the Swedish Academy for the year 1771. C. xxxiii. p. 122.

† Dr. Lewis, in his very useful work on the Arts, has laid down the best construction of this engine, from actual experiment.

periment,



periment, which would not be attended with too much inconvenience. This I have effected by the instrument of which I now send the draft and description; and I found the result of the experiments made with it, fully to answer my expectations.

In the draft the first figure represents the external appearance of this instrument, and the second shews it in section. Plate 1. From A to B in fig. 2, there is a free open communication in the larger tubes. The part C E D is an open truncated cone, ending in a tube E F, one third of the diameter of that which furrounds it, and which reaches into the larger tube the length of two of the diameters of the latter.

There is a scale with the draft for fig. 1 and 2, which marks the proportion of the small instrument I had first made. But as the size of any other must vary according to the uses for which it is intended, I have only set down the number of proportional parts, and have not mentioned their value, which may be encreased at pleasure.

In using this instrument, the base C D of the cone must be turned towards the wind, which when it blows even moderately, will then cause a very sensible current of air to pass up from A to B through the larger tubes. The small instrument, which I tried at a time when the wind was low, when placed as above directed, opposite the opening of the upper sash of a window let down about two inches, acted with such power, as to draw the flame of a candle considerably into the tube A, held horizontally at some distance from it, and made the tube B, by which I supported it, so hot in a short time, that it was painful to hold it.

This experiment I repeated before many gentlemen always with the same result; and which at least entitles the instrument to a trial on a larger scale for useful purposes, in which I have no doubt of its succeeding.

The uses to which this instrument may be applied, are of no small importance, even so far as they have occurred to me; others may yet be found out which I have not thought of, those which I have thought of are as follows:

1st. It will do extremely well for extracting the unwholesome air from mines, when it is made of a size sufficiently large.

2d. It will also be particularly convenient in ships for changing the air in them, rendered unfit for respiration by the breath and perspiration of a numerous crew, by the effluvia from provisions, bilge water, &c.; and if its efficacy may be judged of in this respect, from experiments made on a small scale, it would with a moderate wind entirely change the air in a large vessel in the course of two or three hours, even when made of no great size.

3d. It will considerably encrease the force of the draft in a blast furnace, if placed on the top of its chimney as hereafter directed, for chimneys in general.

4th. It will serve as a ventilator for corn, stores, magazines, hospitals, and crowded rooms.

5th. It is particularly well calculated to prevent or cure the smoking of chimnies, (which inconvenience frequently renders houses so uncomfortable, as to induce the owners to quit

them;

them; to say nothing of the injurious effects of smoke received into delicate lungs. One cause especially of smoking chimneys it removes, for which no effectual cure has been before discovered.

Dr. Franklin, to whom the world is so much indebted for his many useful discoveries in natural philosophy, and the arts, has given two very valuable papers on the subject of smoking chimnies, in the Transactions of the Philadelphia Society. In these he has recited nine causes, which occasion chimneys to smoke, amongst which that of high winds blowing the smoke down the funnel is a chief one: for all which he has proposed effectual remedies; except for this last mentioned alone, for which, he acknowledges that there was no certain remedy discovered; we may therefore look on such a remedy as an object the more valuable, as it compleats what the Doctor began: such a remedy is the instrument described in this paper, which by removing this evil leaves no defect incident to chimneys that cannot be obviated; and it is so effectual for this purpose, that from the very principles of its construction, the stronger the wind blows, the better will the chimney draw, thus changing what was formerly an evil into an actual benefit.

The manner in which the instrument should be used for this purpose, is one with which we are already familiar. It should be placed on the top of the chimney, in the same manner in which the contrivance, (intended for the same purpose, but very ineffectual) called the Boar's Head, is usually put up: and should like it be supported on a pivot, so as to turn round with its back to the wind, that the blast might fully enter its conical opening.

To prevent the external air from being drawn up between the turning tube and the fixed one, when placed on the chimney, some soft leather, or canvas, should be fastened round the outside of the lower end of the turning tube, which surrounds the fixed tube; this leather or canvas, which should hang down two or three inches below the interval between the tubes, will act as a species of valve to prevent the outer air from entering, and will thus cause the whole effect of the instrument to operate in increasing the draft of the chimney.

There is another method of effecting this last purpose, more certain in its mode of acting, but which from its requiring a much greater expence, cannot be recommended for general use; this is represented in fig. 3. which figure is on a smaller scale than the other two, and is only intended to shew the best method of fixing the instrument on a chimney: it is drawn in section to exhibit the internal structure. In it A represents the fixed part to be bedded in mortar on the top of the chimney in the usual manner. B represents the moveable part, which turns with the wind, so that the conical opening G shall be always opposite to the blast. E is the upright rod on whose point B turns round; it is fastened below to the two bars DD, which are fixed across in the lower immoveable tube A, and passes through a collar in the bar H, which is fastened at its extremities to the moveable tube B. By this means the rod preserves its perpendicular position; and the part of the moveable tube, which surrounds the upper part of the fixed one, will revolve round it very close without coming in contact in any part.



CC is an external tube, closely folded or cemented to the fixed tube A, so that, with its upper part, it may form a deep groove of four or five inches, in which the lower extremity of the moveable tube may freely revolve: this groove should be filled with mercury, high enough to cover the bottom of the moveable tube about half an inch; which contrivance will effectually prevent the outer air from being drawn in between the tubes, without impeding the motion of the upper one in the least. FF is a small projection fastened to the moveable tube, to prevent the rain or dust from falling into the groove.

The Blast Ventilator would be particularly serviceable to ship chimnies above all others. Any one who has ever been at sea, must have observed the difficulty of preventing ship-chimnies from smoking when the wind rises to any height, and what an advantage any contrivance would be that would prevent this inconvenience; and that this instrument would be quite effectual for this purpose, (there is every reason to believe from the experiments already made) and even make those chimnies draw better as the wind blew harder.

The instrument represented in the two first figures would be the fittest for this purpose, any of the men on deck could easily shift it about as the wind changed; and as this way of using it might be made for a few shillings, the expence will be no object to prevent ship-owners from giving it a fair trial.

It is scarcely necessary to mention, that it would do equally well for the same purpose in canal passage-boats, which are often as much incommoded by smoke as ships are; a mode of conveyance to which in some situations, elegance is added to ease and cheapness, and which well deserves farther extension on the English canals.

The manner in which the wind operates in this apparatus, to cause a current of air through the upright tube, is most probably the following:

When the conical opening CD, Fig. 2, is turned to the wind, the portion of the current of air which enters this opening, will be compressed against the sides of the cone, as it passes on, more and more, till it comes to E; and after it has passed F, the extremity of the small tube, it will begin to expand itself; but as it moves forward while thus expanding, the union of the two motions will cause it to form the figure of a cone or conoid, somewhat similar to that by which it entered, which is marked by the dotted lines terminating at G and H.

This conical blast striking obliquely the air before it in the tube B, will cause it to move forward only, its lateral motion being prevented by the sides of the tube; and the pressure of the atmosphere forcing the air forward at A, to replace what has been removed at B by the conical blast, will of course cause the current of air from A through B. already mentioned.

In situations where coals may be had cheap, steam may be made to operate on the blast ventilator instead of wind, for many useful purposes, by a contrivance somewhat similar to the eolipile; with some alteration as to its form, but preserving the same principle; particularly steam might be used with this instrument so as to force air on a blast furnace, and would:

would do best for smelting iron, where some degree of dampness in the blast is rather beneficial.

To describe the apparatus minutely for this purpose, would swell this paper to too great a bulk; but of its possibility I could easily convince any one, and would engage to construct it, in such a situation as before mentioned, at an expence comparatively trifling to any other mode of causing a blast, where a fall of water could not be procured.

Even where fuel is not very cheap, yet steam may be applied to the purpose of ventilation with this instrument, in the manner hinted at here, to great advantage, in those situations where labour is difficult to be procured, or inconvenient to be used; or where habits and prejudice operate strongly against working manual ventilators. This last is too frequently the case aboard men of war, and other ships, though in no other place is ventilation so necessary to preserve health, and secure provisions from decay.

An apparatus for ventilating ships in this manner might easily be fixed beside the ship's boiler, to be used in calms, when the wind would not operate on the instrument before described, and which at other times might be applied to boil water for various uses; and the ship's boiler itself might be so adjusted as to serve for this purpose, without preventing or impeding its use in preparing provisions.

To conclude, I will just mention a matter this subject has suggested of some importance, though somewhat differing from what has already been treated of.

From observing the effects of the conical opening in the blast ventilator, it occurs, that walls might be so constructed about a windmill, as to operate in a similar manner, and that thus the wind might be conducted with increased force, to operate in any direction required on a fixed wind wheel, though it blew from various directions; and that a windmill thus constructed, would have the additional advantage of being capable to be regulated or stopped at pleasure, in a manner somewhat similar to a water-mill. This mode of constructing those mills would prevent many fatal accidents; and would not cost so much as might at first seem, as the granaries, and other buildings annexed to them, might be so arranged, as in a great measure to answer the purpose of the walls proposed to be built\*.

### III.

*Experiments on Whinstone and Lava.* By Sir JAMES HALL, Bart. F.R.S. & F.A.S.  
Edin †.

THE experiments described in this paper were suggested to me many years ago, when employed in studying the *geological* system of the late Dr. Hutton, by the following plausible objection, to which it seems liable.

\* On the subject of this paper, see Phil. Journ. I. II. Art. Venturi. N.

† Read before the Royal Society of Edinburgh, and communicated by the author. An abridged account of the contents of this paper was given in our Journal II, 285.



Granite, porphyry, and basalt, are supposed, by Dr. Hutton, to have flowed in a state of perfect fusion into their present position; but their internal structure, being universally rough and stony, appears to contradict this hypothesis; for the result of the fusion of earthy substances, hitherto observed in our experiments, either is glass, or possesses, in some degree, the vitreous character.

This objection, however, loses much of its force, when we attend to the peculiar circumstances under which, according to this theory, the action of heat was exerted. These substances, when in fusion, and long after their congelation, are supposed to have occupied a subterraneous position far below what was then the surface of the earth; and Dr. Hutton has ascribed to the modification of heat, occasioned by the pressure of the superincumbent mass, many important phenomena of the mineral kingdom, which he has thus reconciled to his system.

One necessary consequence of the position of these bodies, seems, however, to have been overlooked by Dr. Hutton himself; I mean, that, after their fusion, they must have cooled very slowly; and it appeared to me probable, on that account, that, during their congelation, a crystallization had taken place, with more or less regularity, producing the stony and crystallized structure, common to all unstratified substances, from the large grained granite, to the fine grained and almost homogeneous basalt. This conjecture derived additional probability from an accident similar to those formerly observed by Mr. Keir, which had just happened at Leith: a large glass-house pot, filled with green bottle glass in fusion, having cooled slowly, its contents had lost every character of glass, and had completely assumed the stony structure.

These views made part of a paper which I had the honour of laying before this Society in 1790\*; and about the same time I determined to submit my opinions to the test of experiment. I communicated this intention to all my friends, and in particular to Dr. Hutton; from him, however, I received but little encouragement. He was impressed with the idea, that the heat to which the mineral kingdom has been exposed was of such intensity, as to lie far beyond the reach of our imitation, and that the operations of nature were performed on so great a scale, compared to that of our experiments, that no inference could properly be drawn from the one to the other. He has since expressed the same sentiments in one of his late publications, (*Theory of the Earth*, vol. I. p. 251.), where he censures those who “judge of the great operations of the mineral kingdom, from having “kindled a fire, and looked into the bottom of a little crucible.”

But, notwithstanding my veneration for Dr. Hutton, I could not help differing from him on this occasion: for, granting that these substances, when in fusion, were acted upon by a heat of ever so great intensity, it is certain, nevertheless, that many of them must have congealed in moderate temperatures, since many are easily fusible in our furnaces; for it

\* Particular reasons induced me not to publish this paper at full length; but, wishing to preserve a record of some opinions peculiar to myself which it contained, I introduced a short abstract of it into the History of the Transactions.

is impossible that a substance should congeal at a higher point than that at which it may afterwards be melted. If, then, these phenomena depend upon the circumstances of congelation, the imitation of the natural process is an object which may be pursued with rational expectation of success: and could we succeed in a few examples on a small scale, and with easily fusible substances, we should be entitled to extend the theory, by analogy, to such as, by their bulk, or by the refractory nature of their composition, could not be subjected to our experiments. It is thus that the astronomer, by observing the effects of gravitation on a little pendulum, is enabled to estimate the influence of that principle on the heavenly bodies, and thus to extend the range of accurate science to the extreme limits of the solar system.

Encouraged by this reasoning, I began my projected series of experiments in the course of the same year (1790), with very promising appearances of success. I found that I could command the result which had occurred accidentally at the glass-house; for, by means of slow cooling, I converted bottle glass, after fusion, into a stony substance, which again, by the application of strong heat, and subsequent rapid cooling, I restored to the state of perfect glass. This operation I performed repeatedly with the same specimen, so as to ascertain that the character of the result was stony or vitreous, according to the mode of its cooling.

Some peculiar circumstances interrupted the prosecution of these experiments till last winter, when I determined to resume them. Deliberating on the substance most proper to submit to experiment on this occasion, I was decided by the advice of Dr. Hope\*, well known by his discovery of the earth of strontites, to give the preference to whinstone.

The term whinstone, as used in most parts of Scotland, denotes a numerous class of stones, distinguished in other countries by the names of basaltes, trap, wacken, grüstein and porphyry. As they are, in my opinion, mere varieties of the same class, I conceive that they ought to be connected by some common name, and have made use of this, already familiar to us, and which seems liable to no objection, since it is not confined to any particular species †.

The following experiments were performed with various kinds of whinstone, and have likewise been extended to lava. To investigate the relation between these two classes of substances, seems, in the present state of geology, an object of considerable importance; for they resemble each other in so many respects, that we are naturally led to ascribe the

\* In the course of last winter, when I first thought of resuming my experiments, I proposed to this gentleman, that, in imitation of a practice, common in the Academy of Sciences of Paris, we should perform them in company. To this proposal he cheerfully agreed; but, before any experiments had been begun, he found himself so much occupied by professional duties, that he could not bestow upon the subject the time which it necessarily required; and we gave up the idea of working in company.

† In characterising the particular specimens, I have adopted, with scarcely any variation, descriptions drawn up by Dr. Kennedy, whose name I shall have occasion frequently to mention in the course of this paper. In the employment of terms, we have profited by the advice of Mr. Deriabin, a gentleman well versed in the language of the Wernerian school.



formation of both to the same cause, and to believe that whinstone, as well as lava, has been exposed to the action of heat. In the course of the paper, I shall mention several accidental results, which, if considered separately, might seem unworthy of notice, but which, by affording the means of comparison between the two classes, are of great service in the general investigation.

The whinstone first employed was taken from a quarry\* near the Dean, on the water of Leith, in the neighbourhood of Edinburgh. This stone is an aggregate of black and greenish-black hornblend, intimately mixed with a pale reddish-brown matter, which has some resemblance to felspar, but is far more fusible. Both substances are imperfectly and confusedly crystallized in minute grains. The hornblend is in the greatest proportion; and its fracture appears to be striated, though in some parts foliated; that of the reddish-brown matter is foliated. The fracture of the stone *en masse* is uneven, and it abounds in small facettes, which have some degree of lustre. It may be scratched, though with difficulty, by a knife, and gives an earthy smell when breathed on. It frequently contains small specks of pyrites.

On the 17th of January 1798, I introduced a black lead crucible, filled with fragments of this stone, into the great reverberating furnace at Mr. Barker's iron foundery. In about a quarter of an hour, I found that the substance had entered into fusion, and was agitated by a strong ebullition. I removed the crucible, and allowed it to cool rapidly. The result was a black glass, with a tolerably clean fracture, interrupted however by some specks.

In subsequent experiments, I endeavoured, by slow cooling after fusion, to prevent the whinstone from becoming vitreous, and to compel it to resume its original character by crystallization. In this I so far succeeded as to obtain a substance, which was not glass, though it did not possess the properties of whinstone. The production of this intermediate substance, which much resembled the liver of an animal, is accompanied with some curious particulars, which I shall enumerate and explain in another part of this paper. On some occasions, too, I obtained a vitreous mass in which were a multitude of little spheres, having a dull or earthy fracture.

At last, on the 27th of January, I succeeded completely in the object I had in view. A crucible, containing a quantity of whinstone, melted in the manner above described, being removed from the reverberatory, and conveyed rapidly to a large open fire, was immediately surrounded with burning coals, and the fire, after being maintained several hours, was allowed to go out. The crucible, when cold, was broken, and was found to contain a substance, differing in all respects from glass, and in texture completely resembling whinstone. Its fracture was rough, stony and crystalline; and a number of shining facettes were interspersed through the whole mass†. The crystallization was still more apparent in  
cavities

\* Called Bell's Mills Quarry.

† It contained a number of small globules like lead shot, which were found to consist of regulus of iron, reduced from the oxide in the whinstone, by means of the carbonic matter of the black lead crucible.

cavities produced by air bubbles, the internal surface of which was lined with distinct crystals \*.

Having shewn this result to several of my friends, Dr. Hope regretted that the substance, previously to its artificial crystallization, had not been reduced to the state of solid glass; since the adversaries of the system might alledge, that, during the action of heat, the original crystallized texture of the stone had never been completely destroyed. Being convinced of the propriety of this observation, I determined, in future, to reduce the stone first to glass, and to perform the crystallization after a second fusion.

For this purpose, with the assistance of Dr. Kennedy, to whose co-operation I am greatly indebted for the success of all the following experiments, I reduced a quantity of the same whinstone to most perfect black glass. A crucible, filled with fragments of this glass, being then exposed to a heat, which, from previous trials, was judged to be more than sufficient to reduce its contents to fusion, the fire was very gradually lowered till all was cold. I thus expected to obtain a result similar to that last mentioned, but found, to my great surprise, that the fragments had never been in complete fusion, since they still, in a great measure, retained their original shape. This extraordinary fact, which afterwards led to the discovery of some curious properties of whinstone, will be fully accounted for in a subsequent part of the paper.

Another portion of the same glass being perfectly melted by a very strong heat, the temperature was reduced to about 28 of Wedgwood, and was maintained at that pitch during six hours. The result was a perfectly solid mass, crystallized to a certain depth from the outside, though still vitreous in the heart. In another experiment, performed like the last in all respects, except that the heat was maintained at 28 during twelve hours, I obtained a mass entirely crystalline and stony throughout, with facettes appearing in the solid parts, and small crystals shooting into some of the cavities.

Soon after I had communicated these results to Dr. Hope, he performed, with complete success, an experiment similar to the first, in which I had obtained a crystallized substance, by the gradual cooling of the melted stone. The same was likewise, soon afterwards, performed by Mr. Boswell of Auchinleck.

My experiments, already described, were confined to one species of whinstone; but have since been extended to six other varieties. They were all first reduced to glass by the application of a strong heat, and subsequent rapid cooling. After a second fusion they

By this reduction, as a portion of its iron was withdrawn from the mass, and as the relative proportion of the component elements was thus varied, the nature of the original substance could not fail to be changed. I determined, on this account, to lay aside black lead crucibles in future. The propriety of this has since been fully proved; for the first result is found to differ essentially from those obtained in all my subsequent experiments, which were performed with Hessian crucibles, or with others, which, like them, contain no sensible quantity of carbonic matter.

\* I showed this result at a meeting of the Society on 5th of February.

WEIC



were crystallized, by being kept long in a stationary temperature, between 28 and 30. This last operation was best performed in a long and narrow muffle, wholly surrounded with burning coals, according to a practice long followed by Dr. Kennedy, by which the heat could be maintained with so great steadiness as to render the result almost certain.

The fusibilities were determined in an open muffle, in which a fragment of the substance under trial was placed contiguous to a pyrometer piece. As soon as the fragment, in consequence of the gradual rise of heat, had so far softened as to yield to the touch of a bent iron rod, the pyrometer was removed and measured. The fusibilities, thus obtained, in degrees of Wedgwood's scale\*, have been stated in a table, to which I would be understood always to refer. I have distinguished the crystallized substances, obtained from the glasses, by the name of *crystallite*, a term suggested by Dr. Hope. It may be observed in this table, that the original whins soften in a range from 38 to 55; the glasses from 15 to 24, and the artificial crystallites from 32 to 45.

#### No. I. *Whin of Bell's Mills Quarry.*

This stone was the subject of all the foregoing experiments, which were frequently repeated with success on a large scale.

In trying the fusibility of the glass obtained from it, a curious circumstance occurred, which accounts for the unexpected results already mentioned. I had placed in the muffle a long and slender fragment of this glass, with its extremities resting on two supports of clay, and its middle unsupported. Having then increased the temperature by slow degrees, I expected to discover the lowest point of emollescence, by observing when the fragment sunk by its own weight. The muffle having attained a moderate heat, I observed the glass to lose its shape a little. Wishing to see it completely melted, the same heat was continued, but no further change took place. The heat was then raised several degrees, but without effect. At last, being urged still further, the glass sunk down completely between its supports. The pyrometer being then withdrawn, denoted a temperature above 30.

It occurred to me, that, on this occasion, the glass, by the first application of heat, had softened, and then had crystallized, so as to become hard again; that, in crystallizing, it had acquired such infusibility as to yield to no heat under 30. I immediately confirmed this conjecture by the following experiment.

A piece of the same glass, placed in a cup of clay, was introduced into the muffle, heated to 21. In one minute it became quite soft, so as to yield readily to the pressure of an iron rod. After a second minute had elapsed, the fragment, being touched by the rod, was

\* The measurement of the temperatures may be relied upon as accurate; they were determined by two sets of pieces, one purchased by me during the lifetime of the late Mr. Wedgwood, and the other likewise made by him, belonging to Dr. Kennedy. The two sets correspond exactly; and Dr. Kennedy's had, at his request, been carefully examined by the present Mr. Wedgwood, who found them true by his father's original standard.

found to be quite hard, though the temperature had remained stationary. The substance, thus hardened, had undergone a change throughout; it had lost the vitreous character; when broken, it exhibited a fracture like that of porcelain, with little lustre; and its colour was changed from black to dark brown. Being exposed to heat, it was found to be fusible only at 31; that is, it was less fusible than the glass by 13 or 14 degrees.

Numerous and varied experiments have since proved, in the clearest manner, that, in any temperature, from 21 to 28 inclusive, the glass of this whin passes from a soft, or liquid state, to a solid one, in consequence of crystallization; which is differently performed at different points of this range. In the lower points, as at 23, it is rapid and imperfect; in higher points, slower and more complete, every intermediate temperature affording an intermediate result. I likewise found, that crystallization takes place, not only when the heat is stationary, but likewise when rising or sinking, provided its progress through the range just mentioned is not too rapid. Thus, if the heat of the substance, after fusion, exceeds one minute in passing from 21 to 23, or from 23 to 21, the mass will infallibly crystallize, and lose its vitreous character.

These facts enabled me to account for the production of the substance resembling the liver of an animal, which I obtained in my first attempts to crystallize the melted stone. Not being then aware of the temperature proper for complete crystallization, I had allowed it to be passed over rapidly by the descending heat, and I had begun the slow cooling in those lower points, at which the formation of this intermediate substance takes place.

By the same means I was enabled to explain the other unexpected result, which I obtained in endeavouring to convert the glass of this stone into crystallite. The fire applied to the crucible, containing fragments of the glass, had been raised very slowly, which I know to have been the case by some circumstances of the experiment. The glass had softened by the first application of heat, but had crystallized again as the heat gradually rose; so that the substance consolidated, while still so viscid as to retain the original shape of the fragments; at the same time it acquired such infusibility as to resist the application of higher degrees of heat during the rest of the process.

#### No. 2. *Whin of the Rock of Edinburgh Castle.*

This is a basalt of a blackish blue colour. Its grain is fine, and its fracture uneven, partaking of the splintery. It is in general homogeneous, although, in some pieces, a very few minute crystals of hornblend are perceptible. It has some lustre, from a number of small shining facettes; has an earthy smell when breathed on; and gives fire slightly with steel.

The pure glass which this whin yielded, by rapid cooling after a moderate heat, was crystallized in three experiments, and produced masses greatly resembling the original. In one of these, formed on a large scale in the glass-house, the resemblance is so strong, both as to colour and texture, that it would be difficult, or perhaps impossible, to distinguish them,  
but



but for a few minute air bubbles visible in the artificial crystallite. The glass is less fusible than that of No. 1. and seems not to possess the property of producing the liver crystallite.

No. 3. *Whin of the Basaltic Columns on Arthur's Seat, near Edinburgh.*

Its basis is a basalt of a dark grey colour, and uneven fracture. It contains numerous laminar crystals of felspar, which seem to be almost colourless, and have considerable lustre and transparency. It also contains some black hornblend. It has an earthy smell when breathed on, and gives sparks slightly with steel.

In the temperature of 100, or upwards, the whole was changed to pure black glass; but in a more moderate heat, (about 60), the felspar remained unchanged, while the hornblend disappeared, and formed a glass along with the basis of the stone. Both kinds of glass yielded highly characterized crystallites; that last mentioned, having its felspars entire, produced a substance like porphyry, in which the white felspars were embedded in a black crystalline basis. The crystals formed in this basis are so complete in one example, that they are seen projecting into the cavities, and standing erect on the external surface, so as to make it sparkle all over. These black crystals seem to be hornblend of new formation. We have found, by some late experiments, that they are considerably more refractory than the crystallite in which they lie, and are equally infusible with some species of natural hornblend.

No. 4. *Whin from the Neighbourhood of Duddingstone Loch.*

It has for its basis a black basalt of an uneven fracture. In it are embedded augit in numerous crystals, felspar in a smaller proportion, and dispersed grains of olivin. The felspar seems to be greenish-white, with considerable lustre and transparency. The stone gives fire with steel, and has a slight earthy smell when breathed on. Its glass yields a fine grained crystallite, like that of No. 1.

No. 5. *Whin of Salisbury Craig near Edinburgh.*

This species is an aggregate of black hornblend, and of a greenish-white matter, both in minute grains. The greenish-white matter resembles felspar, but is much more fusible. The general characters are nearly the same with those of the specimen already described, No. 1. It has considerable lustre, chiefly from the hornblend; an earthy smell when breathed on; and gives some sparks with steel. Its glass yielded a highly facettied crystallite, approaching to the structure of the original whinstone, No. 4.

No. 6. *Whin from the Water of Leith.*

It is found in great blocks in the bed of the river, and has been brought there no doubt from a mass of the same kind in the mountains above. It consists of black hornblend, and of a whitish matter resembling felspar, as in No. 1. and No. 5. These two substances are nearly in equal proportion, and are confusedly and imperfectly crystallized in minute masses. If the whitish substance were felspar, this stone, as well as that last mentioned, would

be the grüstein of Werner; but this white substance is far more fusible than felspar, and melts at a lower heat than the hornblend, with which it is mixed. It has an earthy smell when breathed on, and may be scratched with difficulty by a knife.

In fusion and crystallization it resembled the other whins. A fragment similar to this in all respects, which I found in the neighbourhood of Edinburgh, manifested so strong a disposition to crystallize, that, though cooled in the open air after fusion, it was found stony in the heart, with a vitreous outside. When crystallized, however, with every precaution, it yielded no remarkable result.

#### No. 7. *Whin of the Basaltic Columns of Staffa.*

I received this specimen from a gentleman who broke it from the original rock. It is basalt of a bluish-black colour. It is fine grained and homogeneous; and its fracture is uneven. It has a small degree of lustre, from a number of minute shining points perceptible in a strong light. It gives an earthy smell when breathed on, and may be scratched with difficulty by a knife. It yielded a perfect and very hard glass, which, in a regulated heat, produced a uniform stony crystallite, greatly resembling the original.

It has thus been shown, that all the whins employed assume, after fusion, a stony character, in consequence of slow cooling; and the success of these experiments, with so many varieties, entitles us to ascribe the same property to the whole class. The arguments, therefore, against the subterraneous fusion of whinstone, derived from its stony character, seem now to be fully refuted.

#### *Experiments on Lava.*

IN the investigation of Dr. Hutton's system, great advantage may be expected from an examination of lavas. They have undoubtedly flowed on the surface by means of heat; and whinstone, according to his hypothesis, having flowed in the bowels of the earth by the influence of the same agent, the two classes ought to possess many properties in common, by which the history of both may be illustrated.

I have been enabled to institute a comparison between them, by means of a cabinet of volcanic productions which I collected in 1785, in company with Dr. J. Home of this Society, on Vesuvius, Ætna, and the Lipari Isles. On this occasion we were greatly assisted by the celebrated M. Dolomieu\*, who accompanied us in part of our expedition. This author complains, in his writings, that travellers, in collecting volcanic productions, have brought away only the superficial scoria of lavas, which nearly resemble each other in all

\* Though I differ widely from this gentleman in many of his theoretical opinions, I cannot too strongly express my admiration of his merit as a natural historian. His descriptions of countries, as well as of minerals, present the most lively representations to the mind of the reader, which, in the numerous instances I have witnessed, are perfectly correct.



cales, and convey no idea of the real character of the lava, which can only be seen in the interior parts of the currents. In forming our collection we scrupulously avoided this error, and chose such specimens only as were the most compact and free from the scorified appearance of the surface.

When these solid lavas are compared with our whinstones, the resemblance between the two classes is not only striking at first sight, but bears the closest examination. They both consist of a stony basis, which frequently contains detached crystals of various substances, such as white felspar and black hornblend. The analogy between the two classes seems to hold through all their varieties; and I am confident that there is not a lava of Mount *Ætna* to which a counterpart may not be produced from the whinstones of Scotland.

This resemblance in external character is accompanied with an agreement no less complete in chemical properties. But before I mention the experiments which tend to prove this agreement, it will be necessary first to examine the opinion of two very celebrated authors concerning lavas. M. Dolomieu and Mr. Kirwan, though they differ widely in many respects, agree in believing, that lavas have never been acted upon by heat of sufficient intensity to produce complete fusion; and endeavour, each by an hypothesis peculiar to himself, to account for their fluidity. The opinion of these gentlemen is of such importance in the present question, and the arguments they have used are so extraordinary, that I must beg leave to quote their words at full length.

M. Dolomieu states his opinion in the following passage, (*Isles Poncez*, p. 7.): “ Il est essentiel de constater, par beaucoup d'exemples et d'observations, quelques vérités que j'ai annoncées il y a plusieurs années, savoir, que le feu des volcans ne dénature pas ordinairement les pierres qu'il a mises en état de fusion; qu'il ne les altère pas au point de ne pouvoir les reconnoître, de ne pas distinguer quelle a pu être la base des laves; que ce feu agit différemment que le feu de nos fourneaux, tel que nous l'employons dans la chimie et dans les arts; qu'il produit dans les laves une fluidité qui n'a aucun rapport avec la fluidité vitreuse, que nous opérons, lorsque nous traitons à grand feu les mêmes matières qui leur servent de base, et lorsque nous voulons rendre aux laves elles mêmes leur fluidité. Celui des volcans n'a point d'intensité; il ne peut pas même vitrifier les substances les plus fusibles, tels que les schorls, qui se trouvent comme parties constituantes dans l'intérieur des laves; il produit la fluidité par une espèce de dissolution par une simple dilation, qui permet aux parties de glisser les unes sur les autres, et peut-être encore par le concours d'une autre matière qui sert de véhicule à la fluidité.”

Mr. Kirwan censures this supposition as strange and inconceivable; but in my opinion, that which he has brought forward is not less so. In the *Elements of Mineralogy*, second edition, vol. I. p. 396. he says: “ Now, there are but three sorts of fusion with which we are acquainted: that which produces porcelain; that which produces enamels and semi-vitrifications; and that which produces glasses. By inspecting lava we shall find that very little of it has been in any of these states; since therefore it has flowed, it is plain it has derived its liquifaction not from the fusion of its own materials, but from that of some

“ foreign substance mixed it. This fact is so plain, that it has even struck M. Dolomieu, in the midst of his prepossessions in favour of some strange inconceivable power, which he attributes to volcanic fire, of melting earthy substances, without effecting an alteration in their sensible qualities. “ I hope,” says he, “ to prove, that lavas contain, in their interior, a combustible matter, which burns and consumes in the same manner as other inflammables,” *Isles Ponce*, 10. Yet he neglects telling us what this matter is; though it plainly appears to be no other than sulphur and bitumen, of which an immense quantity is found in all volcanos, which liquefies in a low degree of heat, and causes all the stony substances to flow that are immersed in it.”

(To be continued.)

#### IV.

*On the Hydraulic Machine erected at the Mines of Schemnitz in Hungary.*

TO MR. NICHOLSON.

SIR,

THE early insertion of the extract which I sent you from the *Micrographia* of Robert Hooke in your last Number, encourages me to request you will give some account, with your opinion, of a machine for raising water at the mines in Hungary, upon the principle of the fountain of Hiero. It is slightly mentioned in many elementary books, but I cannot point out the work in which it was originally published, nor say thing more than that I believe it has not been accurately described in any English work.

I am, &c.

R. B.

*London, Feb. 20, 1800.*

The account was first given to the Royal Academy at Paris by their correspondent, M. Jars, and inserted in their *Memoirs* for the year 1768. The machine was executed in the year 1755, and used to raise the water of a shaft named *Amalie*, in the mines at Schemnitz. Fig. 1, Plate 2, is copied from the engraving of Jars. It is placed at the depth of forty toises beneath the surface of the ground above. The wooden trough A is a kind of reservoir at the end of a channel which conveys the water by which the machine is worked. The pipes are not drawn in the proportion of their lengths, but contracted to the space of the design. B B conveys the water from it into the reservoir D, which is twenty-two toises lower than A. This pipe descends very nearly to the bottom of D, as is shewn by the dotted line, with the intention that the included air shall be forced to ascend



ascend and pass through the pipes H into another reservoir I still lower. The pipes N, through which the subterraneous waters are raised, are sixteen toises in height. They begin very near the bottom of the vessel I, in order that the air from that reservoir may not escape until all the water has been raised.

The pipes B and N are four and a half (French) inches in diameter within; but those H for the air are only three inches in diameter at issuing from the reservoir D, and two and a half when they enter the reservoir I, and this also is the diameter of the pipe P.

The wooden trough L is the termination of a trough or channel from another engine, which raises the waters from a yet greater depth. From this proceeds a pipe with a cock K, in order that the water from L may either be admitted into I, or intercepted at pleasure.

The reservoir D, which, as well as I, is made of an alloy of copper and tin, is of double the capacity of I. It is eight feet and a half high, and five feet diameter inside measure, and its thickness is two inches. The lower reservoir I is four feet in diameter, and six feet and a half high inside measure, and its thickness one inch and a half. Each of these vessels was cast in three pieces, as the drawing shews, which are joined by flanches and screws, a ring of lead and another of leather being placed between, to secure the joint, and prevent the transmission of any fluid. M. Jars observes, that the pipes would have been better if connected in the same manner; but the practice is to drive the ends of the pipes into hollow cylinders of very dry wood, iron bound with three hoops. These answer very well, and are of considerable durability.

The cocks C, E, K are screwed in their places by caps or covers fastened down with screws.

When the machine is to be put into action, all the cocks are closed, and as the reservoir A is always full, the pipe B is also full, as far as the cock C. The reservoir L is also constantly full of water from the mine which is to be raised sixteen toises to O. For this purpose the cock K is opened, and the water flows into I, the air being suffered to escape from that vessel by turning the cock M. The vessel is known to be full by the emission of water through P; at which instant both the cocks K and M are closed. Immediately afterwards the cocks C and G are opened, and the water from A entering B, compresses the included air, and forces it through the pipes H, where it presses on the surface of the water contained in the inferior reservoir I, and forces that fluid to ascend through N to O, or the Adit through which it is discharged. This water being raised, and the reservoir I being empty, the cocks C and G are shut, and E and F is then opened, the first to suffer the contents of D to flow off, and the second to accelerate the discharge, by admitting the external air. Both these cocks are again closed as soon as the evacuation is completed. During this last operation, another man below opens the cocks K and M, by which the included air issues with great force through P, and the water from L again fills the reservoir I as at first. The operations of closing K and M, and opening C and G, being therefore again repeated, the contents of I are again forced up to, and thus the engine may be kept continually at work.

Each alternation employs about three minutes, and raises between twenty-nine and thirty cubic feet of water, which, in the course of the natural day, amounts to twelve or thirteen thousand cubic feet of water, raised by the fall of about double the quantity through a somewhat greater height. Two men\* are required to attend it; but M. Jars seems to think, that on account of the simplicity and cheapness of its construction, and the little wear and tear, together with the facility with which it may be made to work and stop for very short periods of time, it would be of great value in such places as afford the requisite fall of superior water, and do not require a higher single lift than fifteen or twenty toises (or fathoms.)

The fall of twenty-two toises given to the superior water is not necessary for raising the column of sixteen, though it is profitable, by increasing the velocity of ascent. But it was at first intended to force the lower water as high as twenty-one toises, which could not be performed with the apparatus then set up. For at the very first introduction of water into the vessel D, it burst with an effect which sufficiently shewed the power of such a column of water compressing a mass of air which, by its elasticity, was disposed to give a vibratory or jerking motion to the agent.

When the machine is near the end of its action, that is to say, when nearly the whole of the water has been raised out of the reservoir I, if the cock M be opened to give vent to the compressed air, and a hat, or miner's bonnet, be presented to the aperture P, the aqueous vapours dispersed through the compressed air, and perhaps also, says M. J. part of those of the external air, are condensed on the bonnet, in the form of very white and compact ice, very much resembling hail, and not easily separated from the bonnet: it soon melts, which is not to be wondered at, as the temperature of the place itself is not cold. Messrs. du Hamel and Jars remained in Hungary from January to July, 1758, and observed the same phenomenon at all seasons; but as they had not a thermometer, they could not make a number of experiments, which might have been of value in the investigation of the subject.

It is observable, that the air issues out with such impetuosity, that the workman could not hold the bonnet at the distance of a few inches from the aperture, as he does in this experiment, if he were not supported behind. The ice is much more compact if the cock be only in part opened.

On the above, which is translated from the Memoir of M. Jars, with very little abridgement, I have scarcely any additional remark to make. The utility of this engine, under the circumstances here stated, is sufficiently clear, and its applications might become more extensive, if the improvements suggested in the preceding note were added. In particular, where a stream flows with the possibility of carrying off part of the water by a deep

\* It seems very easy to connect the levers of the cocks above and below, so as to require only one man to work the whole set: and, indeed, there would be little difficulty in making the machine work itself safely without any attendant except to set it off at first, or stop it when requisite.---N.



shaft or drain, it would be practicable by the fall of part to raise the remainder to the top of an house or building for domestic or other purposes. In this case it would be requisite to place the first vessel D at the bottom of the shaft, and the second I at the level of the stream, and the operation of this inverted engine would force water nearly as high above that level as the depth of the shaft might be below it.

If the ascending and descending columns were of equal length, they would be in equilibrio, and no more water would rise than just to fill the lower tube. The velocity of ascent, supposing sufficient water way, and other circumstances, such as the inflexions of the pipes, to be properly disposed, will depend on the excess of length in the descending column, and this excess is capable of a maximum relative to the quantity raised to a given height, or as engineers call it, the effect. The longer the lift, the less the quantity raised, supposing the upper reservoir to be of a given magnitude, the densities of air being inversely as the pressures, or length of the pressing column, and the quantity raised being equal to that of the air in its compressed state. Whence it follows, that the whole quantity raised will be less the fewer the lifts.

I do not understand the hypothesis upon which this machine was constructed. The reservoirs have their capacity as 2 to 1. But the air subjected to the re-action of a column of 16 toises, besides the common pressure, would sustain the pressure of four atmospheres, and therefore be condensed into one-fourth the space. If there be no fallacy in this plain remark, it must follow, that the lower vessel I was only half emptied when the stream ceased to be afforded at O, and, consequently, the effect was only half what is here stated.

That air condensed to one-fourth should take up and dissolve more water than in its ordinary rare state, and afterwards deposit it when it recovered its original dimensions, is consentaneous with other well known facts: and the modern theorists will easily apply the doctrine of latent heat, or the increased capacity of expanded air, to account for the phenomenon of its robbing the water not only of the heat which maintained its state of elastic solution, but even that which would have been requisite to keep up the state of common dense fluidity.

W. N.

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## V.

*An Account of the Pearl Fishery in the Gulph of Manar, in March and April 1797. By  
HENRY J. LE BECK, Esq.*

*(Concluded from Vol. III. page 547.)*

THE diving stone is a piece of coarse granite, a foot long, six inches thick, and of a pyramidal shape, rounded at the top and bottom. A large hair rope is put through a hole in the top. Some of the divers use another kind of stone shaped like a half moon, to bind.

bind round their belly, so that their feet may be free. At present these are articles of trade at *Condatchey*. The most common, or pyramidical stone, generally weighs about thirty pounds. If a boat has more than five of them, the crew are either corporally punished or fined.

The diving, both at *Ceylon* and at *Tutucorin*, is not attended with so many difficulties as authors imagine. The divers, consisting of different casts and religions, (though chiefly of *Parrawer* \* and *Musselmans*,) neither make their bodies smooth with oil, nor do they stop their ears, mouths, or noses with any thing, to prevent the entrance of salt water. They are ignorant of the utility of diving bells, bladders, and double flexible pipes. According to the injunctions of the shark conjurer they use no food while at work, nor till they return on shore, and have bathed themselves in fresh water. These *Indians*, accustomed to dive from their earliest infancy, fearlessly descend to the bottom in a depth of, from five to ten fathoms in search of treasures. By two cords a diving stone and a net are connected with the boat. The diver putting the toes of his right foot on the hair rope of the diving stone, and those of his left on the net, seizes the two cords with one hand, and shutting his nostrils with the other, plunges into the water. On reaching the bottom, he hangs the net round his neck, and collects into it the pearl shells as fast as possible, during the time he finds himself able to remain under water, which usually is about two minutes. He then resumes his former posture, and making a signal, by pulling the cords, he is immediately lifted into the boat. On emerging from the sea, he discharges a quantity of water from his mouth and nose, and those who have not been long enured to diving frequently discharge some blood; but this does not prevent them from diving again in their turn. When the first five divers come up and are respiring, the other five are going down with the same stones. Each brings up about one hundred oysters in his net, and if not interrupted by any accident, may make fifty trips in a forenoon. They and the boat's crew get generally from the owner, instead of money, a fourth of the quantity which they bring on shore; but some are paid in cash, according to agreement.

The most skilful divers come from *Colliß*, on the coast of *Malabar*; some of them are so much exercised in the art, as to be able to perform it without the assistance of the usual weight; and for a handsome reward will remain under water for the space of seven minutes; this I saw performed by a *Caffry* boy, belonging to a citizen at *Karical*, who had often frequented the fisheries of these banks. Though Dr. Halley deems this impossible, daily experience convinces us, that by long practice any man may bring himself to remain under water above a couple of minutes. How much the inhabitants of the South Sea islands distinguish themselves in diving we learn from several accounts; and who will not be surprised at the wonderful *Sicilian* diver *Nicholas*, surnamed the Fish †?

\* Fishermen of the Catholic religion.

† According to Kircher, he fell a victim amongst the *Polypes* in the gulph of *Charybdis*, on his plunging, for the second time, in its dangerous whirlpool, both to satisfy the curiosity of his king, *Frederic*, and his inclination for wealth. I will not pretend to determine, how far this account has been exaggerated.



Every one of the divers, and even the most expert, entertain a great dread of the sharks, and will not, on any account, descend until the conjurer has performed his ceremonies. This prejudice is so deeply rooted in their minds, that the government was obliged to keep two such conjurers always in their pay, to remove the fears of their divers. Thirteen of these men were now at the fishery from *Ceylon* and the coast, to profit by the superstitious folly of these deluded people. They are called in *Tamul*, *Pillāl Kadiār*, which signifies one who binds the sharks and prevents them from doing mischief.

The manner of enchanting consists in a number of prayers learned by heart, that nobody, probably not even the conjurer himself, understands, which he, standing on the shore, continues muttering and grumbling from sun rise until the boats return; during this period, they are obliged to abstain from food and sleep, otherwise their prayers would have no avail; they are, however, allowed to drink, which privilege they indulge in a high degree, and are frequently so giddy, as to be rendered very unfit for devotion. Some of the conjurers accompany the divers in their boats, which pleases them very much, as they have their protectors near at hand. Nevertheless, I was told, that in one of the preceding fisheries, a diver lost his leg by a shark, and when the head conjurer was called to an account for the accident, he replied that an old witch had just come from the coast, who, from envy and malice, had caused this disaster, by a counter-conjuration, which made fruitless his skill, and of which he was informed too late; but he afterwards shewed his superiority by enchanting the poor sharks so effectually, that though they appeared in the midst of the divers, they were unable to open their mouths. During my stay at *Condatchey*, no accident of this kind happened. If a shark is seen, the divers immediately make a signal, which, on perceiving, all the boats return instantly. A diver who trod upon a hammer oyster, and was somewhat wounded, thought he was bit by a shark, consequently made the usual signal, which caused many boats to return; for which mistake he was afterwards punished.

The owners of the boats\* sometimes sell their oysters, and at other times open them on their own account. In the latter case some put them on mats in a square, surrounded with a fence; others dig holes of almost a foot deep, and throw them in till the animal dies; after which they open the shells and take out the pearls with more ease. Even these squares and holes are sold by auction after the fishery is finished, as pearls often remain there, mixed with the sand.

In spite of every care, tricks in picking out the pearls from the oysters can hardly be prevented. In this the natives are extremely dexterous. The following is one mode they put in practice to effect their purpose: when a boat owner employs a number of hired people to collect pearls, he places over them an inspector of his own, in whom he can confide; these hirelings previously agree that one of them shall play the part of a thief, and bear the punishment, to give his comrades an opportunity of pilfering. If one of the gang happens

\* These are the individuals which farm one or more boats from the renter; and though they are in possession of them only during the fishery, they are commonly called the owners of the boats.



to meet with a large pearl, he makes a sign to his accomplice, who instantly conveys away one of small value, purposely, in such a manner as to attract notice. On this the inspector and the rest of the men take the pearl from him: he is then punished and turned out of their company. In the mean time, while he is making a dreadful uproar, the real thief secures the valuable pearl, and afterwards the booty is shared with him who suffered for them all. Besides tricks like these the boat owners and purchasers often lose many of the best pearls, while the dony is returning from the bank; for, as long as the animal is alive and untouched, the shells are frequently open near an inch; and if any of them contain a large pearl, it is easily discovered and taken out by means of a small piece of stiff grass or bit of stick, without hurting the pearl fish. In this practice they are extremely expert. Some of them were discovered whilst I was there, and received their due punishment.

Gmelin asks if the animal of the *mytilus margaritiferus* is an *ascidia*? See Linn. Syst. Nat. tom. I. p. vi. 3350. This induces me to believe that it has never yet been accurately described; it does not resemble the *ascidia* of Linnæus, and may, perhaps, form a new genus. It is fastened to the upper and lower shells by two white flat pieces of muscular substance, which are called by Houttuin \* *ears*, and extend about two inches from the thick part of the body, growing gradually thinner. The extremity of each *ear* lies loose, and is surrounded by a double brown fringed line. These lie almost the third of an inch from the outer part of the shell, and are continually moved by the animal. Next to these, above and below, are situated two other double fringed moveable substances, like the branchiæ of a fish. These *ears* and *fringes* are joined to a cylindrical piece of flesh; of the size of a man's thumb, which is harder and of a more muscular nature than the rest of the body. It lies about the centre of the shells, and is firmly attached to the middle of each. This, in fact, is that part of the pearl fish which serves to open and shut the shells. Where this column is fastened, we find on the flesh deep impressions, and on the shell various nodes of round or oblong forms, like imperfect pearls. Between this part, and the hinge (*cardo*), lies the principal body of the animal, separated from the rest, and shaped like a bag. The mouth is near the hinge of the shell, enveloped in a veil, and has a double flap or lip on each side; from thence we observe the throat, (œsophagus) descending like a thread to the stomach. Close to the mouth there is a carved brownish tongue, half an inch in length, with an obtuse point; on the concave side of this descends a furrow, which the animal opens and shuts, and probably uses to convey food to its mouth †. Near its middle are

\* Vide Houtt. Nat. Hist. Vol. I. p. xv. p. 381, seq.

† The depth at which the pearl fish generally is to be found, hindered me from paying any attention to the locomotive power, which I have not the least doubt it possesses, using for this purpose its tongue. This conjecture is strengthened by the accurate observations made on *muscles* by the celebrated Reaumur, in which he found that this body serves them as a leg or arm, to move from one place to another. Though the divers are very ignorant with regard to the œconomy of the pearl fish, this changing of habitation has been long since observed by them. They alledge, that it alters its abode when disturbed by an enemy or in search of food. In the former case they say it commonly descends from the summit of the bank to its declivity.

two bluish spots, which seem to be the eyes. In a pretty deep hole, near the base of the tongue, lies the beard (*byssus*), fastened by two fleshy roots, and consisting of almost one hundred fibres, each an inch long, of a dark green colour, with a metallic lustre; they are undivided, parallel, and flattened. In general the *byssus* is more than three quarters of an inch, without the cleft (*rima*); but if the animal is disturbed, it contracts it considerably. The top of each of these threads terminates in a circular gland or head, like the *stigma* of many plants. With this *byssus* they fasten themselves to rocks, corals, and other solid bodies; by it the young pearl fish cling to the old ones, and with it the animal procures its food, by extending and contracting it at pleasure. Small shell fish, on which they partly live, are often found clinging to the former. The stomach lies close to the root of the beard, and has, on its lower side, a protracted obtuse point. Above the stomach are two small red bodies, like lungs; and from the stomach goes a long channel or gut, which takes a circuit round the muscular column above-mentioned, and ends in the anus, which lies opposite to the mouth, and is covered with a small thin leaf, like a flap. Though the natives pretend to distinguish the sexes, by the appearance of the shell, I could not find any genitalia. The large flat ones they call males, and those that are thick, concave, and vaulted, they call females, or *pedoo-chippy*; but, on a close inspection, I could not observe any visible sexual difference.

It is remarkable that some of these animals are as red as blood, and that the inside of the shell has the same colour, with the usual pearly lustre, though my servants found a reddish pearl in an oyster of this colour; yet such an event is very rare. The divers attribute this redness to the sickness of the pearl fish; though it is most probable that they had it from their first existence. In the shade they will live twenty-four hours after being taken out of the water. This animal is eaten by the lower class of *Indians*, either fresh in their curries, or cured by drying; in which state they are exported to the coast; though I do not think them by any means palatable.

Within a mother of pearl shell I found thirteen *murices nudati* (vide Chemnitz's New System, Cabt. vol. XI. tab. 192, f. 1851 and 1852), the largest of which was three quarters of an inch long; but as many of them were putrid, and the pearl fish itself dead, I could not ascertain whether they had crept in as enemies, or were drawn in by the animal itself. At any rate turtles and crabs are inimical to the animals, and a small living crab was found in one of them.

The pearls are only in the softer part of the animal, and never in that firm muscular column above-mentioned. We find them in general near the earth, and on both sides of the mouth. The natives entertain the same foolish opinion concerning the formation of the pearl which the ancients did. They suppose them formed from dew-drops in connection with sun-beams. A *Brâhmen* informed me that it was recorded in one of his *San scrit* books, that the pearls are formed in the month of *May* at the appearance of the *Sooatee* star (one of their twenty-seven constellations) when the oysters come up to the surface of the



water, to catch the drops of rain. One of the most celebrated conchologists\*, supposes that the pearl is formed by the oyster in order to defend itself from the attacks of the *pholades* and *boreworms*. But we may be assured that in this supposition he is mistaken, for although these animals often penetrate the outer layers of the pearl shell, and there occasion hollow nodes, yet, on examination, it will be found, that they are never able to pierce the firm layer, with which the inside of the shell is lined. How can the pearls be formed as a defence against exterior worms, when, even on shells that contain them, no worm-holes are to be seen? It is, therefore, more probable these worms take up their habitations in the nodes, in order to protect themselves from the attacks of an enemy, than that they are capable of preying on an animal, so well defended as the pearl-fish is. It is unnecessary to repeat the various opinions and hypotheses of other modern authors; it is much easier to criticise them, than to substitute in their place a more rational theory. That of Reaumur, mentioned in the memoirs of the *French Academy* for 1712, is the most probable, viz. that the pearls are formed like bezoars and other stones in different animals, and are apparently the effects of a disease. In short it is very evident, that the pearl is formed by an extravasation of a glutinous juice either within the body, or on the surface of the animal: the former case is the most common. Between one and two hundred pearls have been found within one oyster. Such extravasations may be caused by heterogencous bodies, such as sand, coming in with the food, which the animal, to prevent disagreeable friction, covers with its glutinous matter, and which, as it is successively secreted, forms many regular lamellæ, in the manner of the coats of an onion, or like different strata of bezoars, only much thinner; this is probable, for if we cut through the centre of a pearl, we often find a foreign particle, which ought to be considered as the nucleus, or primary cause of its formation. The loose pearls, may originally have been produced within the body, and on their encrease may have separated and fallen into the cavity of the shell. Those compact ones, fixed to the shells, seem to be produced by similar extravasation, occasioned by the friction of some roughness on the inside of the shell. These and the pearl-like nodes have a different aspect from the pearls, and are of a darker and bluer colour. In one of the former I found a pretty large, true oval pearl, of a very clear water; while the node itself was of a dark blueish colour. The yellow or gold coloured pearl, is the most esteemed by the natives; some have a bright, red, lustre; others are grey or blackish, without any shining appearance, and of no value. Sometimes when the grey lamella of a pearl is taken off, under it is found a beautiful genuine one, but it oftener happens that after having separated the first coat you find a worthless impure pearl. I tried several of them, taking one lamella off after another, and found clear and impure by turns, and in an impure pearl I met with one of a clear water, though in the centre of all I found a foreign particle. The largest and most perfect pearl which I saw during my stay at *Condatchey*, was about the size of a small pistol bullet, though I have been told since my departure, many others of the

\* The Rev. Mr. Chemnitz at *Copenhagen*.



same size have been found. The spotted and irregular ones are sold cheap, and are chiefly used by the native physicians as an ingredient in their medicines.

We may judge with greater or lesser probability by the appearance of the pearl-shell, whether they contain pearls or not. Those that have a thick calcareous crust upon them, to which *serpula* (sea tubes) *Tubuli marini irregulariter intorti*, *Crista-gali Chamar lazuras*, *Lepas tintinabulum*, *Madreporee*, *Millipore*, *Cellipore*, *Gorgontæ*, *Spongiæ*, and other zoophytes are fastened, have arrived at their full growth, and commonly contain the best pearls; but those that appear smooth, contain either none, or small ones only.

Were a naturalist to make an excursion for a few months to *Manâr*, the small island near *Jafna* and the adjacent coast, he would discover many natural curiosities, still buried in obscurity, or that have never been accurately described.

Indeed no place in the *East Indies* abounds more with rare shells, than these: for there they remain undisturbed, by being sheltered from turbulent seas, and the fury of the surf. I will just name a few of them; viz. *Tellina foliaca* Lynn \*, *Tell*, *Spenglerii*, *Arca culculata* †, *Arca Noë*, *solen anatinus* Linn. *Ofrea Isognomum*, *Terebellum*, *albidum*, *striatum*, *Turbo scalaris* ‡ *Bula volva* Linn §, *Vexillum ingritarum*, &c. Amongst the beautiful conc shells: *comus thalassiarachus Anglicanus cullatus* ||, *amadis thalassiarachus*, *con. generalis* Linn. *c. capitaneus* \*\*, *c. miles* ††, *c. sterus muscarum* ‡‡. *c. reteaureum*, *c. glaucus* §§, *c. cercola*, *regia corona murus lapidius*, *canda erminea societas cordium*. There are many other besides those already mentioned, equally valuable and curious.

The great success of the Rev. Doctor John in conchology when at *Tutucorin*, and assisted by G. Angelbeck, with a boat and divers: and the capital collections made by his agents, whom he afterwards sent there with the necessary instructions and apparatus, may be seen in Chemnitz's elegant cabinet of shells in 4to. (with illuminated plates), and how many new species of Zoophytes he discovered, we learn from another German work by Esper at *Erlangen*, the third volume of which is nearly finished.

\* The golden tong.

§ Weaver's shuttle.

†† Garter stamper.

† Mounkscape.

|| Red *English* admiral.

‡‡ Great sand stamper.

‡ Royal staircase.

\*\* Green stamper.

§§ Capl. Gottw.

## VI.

*Process for extracting Sugar from the Beet, communicated in a Letter from Mr. ACHARD to Citizen VAN MONS\*.*

IT is the *beta vulgaris* L. which is proper for making of sugar, though all the varieties are not equally so. That of which the internal part is white: the skin of a pale red, and the root of a long spindle shape, is the best. All the varieties of the beet yield sugar, but this must be chosen when it is to be manufactured to advantage; besides which, it is on the culture principally the quantity of sugar produced by the root depends.

It is ascertained from the operations of extracting sugar from the beet root, which I have performed under the inspection of a committee, nominated by the King of Prussia, that the best method is as follows. The root not peeled, but in the state in which it is dug out of the ground, with no other preparation than that of carefully separating the leaves and the heart, is to be boiled in water till it is soft enough to be pierced with a straw. A short time of ebullition is sufficient to give it this degree of softness, which is well known to the confectioners, who give it to some fruits before they preserve them. The beet when cold is divided and reduced into slices by means of the machine used by farmers to slice potatoes for cattle. A description of this machine is to be found in a publication of Busch, entitled: *Uebersicht der fortschritte in wissenschaften, kunsten, manufacturen und handwerken von Oßern 1796, bis Oßern 1797. Erfurt 1798*: and the engraving which renders the subject clearer is copied in the first plate of my work. This method of dividing the root is the best I have yet found. Two men with this machine can cut nearly 100 pounds into very fine slices in three minutes. To extract the juice from the roots when sliced, they are subjected to the action of a press which should act with force sufficient to extract at first as much juice as possible. The pulp which remains in the press still contains a considerable portion of sugar, which is worth extracting. To effect this it is diluted in a sufficient quantity of water for twelve hours, after which the fluid part is drawn out by pressure. The saccharine matter, after this second extraction, is still sufficiently abundant to afford by fermentation, brandy or vinegar, with profit.

The liquids thus obtained are afterwards mixed and strained through a flannel, and reduced by constant boiling to about two thirds. It is then passed a second time through a woollen cloth, or a piece of cloth, such as is used in sugar refineries; after which the liquor is boiled in a smaller vessel than the first till it is reduced to one half. The liquor is again boiled in a still smaller vessel which gives it the consistency of a liquid syrup. It is necessary to remark, that by endeavouring to give to the syrup a too strong consistency there will be danger of spoiling the whole.

\* Decade. An. VIII. p. 15. No. 15. p. 324.



This syrup poured into shallow earthen vessels, which present a large surface to the air, is to be placed in a stove at the heat of 20 or 30 degrees, or if agreeable at 30 or 40 of Reaumur, in order to crystallize it. During this insensible condensation of the syrup the crystalline incrustation, which is formed at the surface, should be occasionally broken, in order that by favouring the evaporation the product of crystals may be hastened. As soon as it is observed that, instead of the crystalline incrustation, a thick gummy pellicle, not granulated, is formed on the surface of the syrup, it is a sign that the matter does no longer crystallize, but begins to dry, and the evaporation should then be stopped. This residue forms a mixture more or less thick, of a crystalline substance and a fluid viscid matter. In order to separate the crystallized sugar from the gummy extract, they are put together into a sack of wet cloth tied tight, and then by means of a press, gradually applied, the fluid part is to be passed through the cloth, and the sugar remains in the bag. This sugar after drying is a yellow muscovado, composed of regular crystals, which when pulverized form a white powder of which the taste is very good, being sweet and clear, and may be applied to a number of uses for which refined sugar is employed. By the operation of refining, sugar may be made from this muscovado of whatever quality is agreeable, and by repeating it the finest sugar may be had. The waste in this manufacture, that is to say the residual pulp, the syrup or mucilage which passes through the cloth when subjected to the press, the syrup in which the sugar has crystallized, the washings, &c. &c. all these are still very useful, and a considerable quantity of rum or brandy may be obtained from them, which may be used in making up the finest compounds. The muscovado, such as is obtained by the first operation, costs about a gross and a half of Prussia, without reckoning the water which may be had by turning the residues to use. When we add this product, and when the manipulations shall be more perfect, to effect which I shall employ myself this winter, I am persuaded that our European muscovado will only cost half the price, or 9 fennins; and in the countries, where fuel is dearer, at 1 gross \*. The manufacture of spirits from the waste of the sugar is of great importance, as by this means a great saving of corn will be made, and the manufacturing of beet sugar, which delivers Europe from a destructive monopoly becomes still more interesting. I am at present employed in the attempt to discover a method of pouring the juice of the roots when sufficiently condensed into moulds or forms, in order that it may acquire the figure of a sugar loaf, and afterwards by claying become very white at a single operation. I have already found several methods of obtaining this object very speedily. This new manipulation will facilitate the art of sugar-making, and diminish the price still further.

\* The pound in use at Berlin, being 476, 28 grammes, and the value of the gross, or of 12 fennins, being 17 centimes, French money, it follows that the hectogramme does not amount to more than about 5 or 6 centimes, (or the pound, poids de marc, from 28 to 50 centimes). Note of the French editor.- (about 2½ d. per lb. avoirdup.)



## VII.

*Letter to the EDITOR from Mr. WILLIAM HENRY, on his Discovery of a new and improved Method of preparing the Prussiate of Pot-ash; and on the disputed Question, Whether Prussiate of Pot-ash be decomposed by Muriate of Barytes?*

*Manchester, March 23, 1800.*

SIR,

IN consequence of a paper, inserted in your Philosophical Journal, I have been favoured with the following letter from Mr. Kirwan. As the references, which it contains, to authorities on a disputed point, may be interesting to others as well as to myself, I take the liberty of sending you a copy of it for publication; premising, that I have stated to Mr. K. my intention of making you this communication, and that he has not expressed any objection.

“ To Mr. WILLIAM HENRY.

“ DEAR SIR,

“ On looking over Mr. Nicholson’s valuable Philosophical Journal, which, though of ancient date, I had not until lately leisure to inspect, I find, in the 29th No. Vol. ii. p. 170, that you seem to regret I had not mentioned on what grounds I denied the precipitation of barytic earth from muriatic acid by the Prussian alkali, and then suggest that the relation of the Prussian alkali to this earth had not been an object of attention to Klaproth or Pelletier, &c. I hope, therefore, it will be agreeable to you that I should state those grounds. That Prussian alkali, properly prepared, does not precipitate barytic solutions, was first proved by that accurate chemist, Mr. Meyer, of Stetin, as you may see in 2 Crell’s Chemical Annals for 1786, p. 142, 143. As I suppose you have that work, I shall not trouble you with the detail of his experiments. You may, also, see that both Pelletier and Klaproth did treat of this fact, Pelletier affirming the precipitation of this earth by the Prussian alkali (21 Journal des Mines, p. 45); and Klaproth expressly denying it, and asserting that Pelletier was mistaken. (See 2 Klaproth, p. 91.)

“ I am, dear Sir,

“ With high regard and respect, &c.

“ R. KIRWAN.”

“ Dublin, Jan. 20.

Unfortunately it is not in my power to consult any one of the works, to which Mr. K. has kindly referred me; and highly as I respect the authorities, to which he has appealed, yet I did not feel disposed to acknowledge myself in an error, till convinced by a circum-

stantial detail of the experiment of Messrs. Meyer and Klaproth. I resolved, therefore, to appeal to the decision of experiments conducted with all possible accuracy and attention.

The question in dispute is, whether or not a double elective affinity be exerted between prussiate of pot-ash and muriate of barytes? If such an affinity really subsists, and produces, on the admixture of the two solutions, two new compounds, one of these (the prussiate of barytes) must necessarily, from its known insolubility, appear under the form of a precipitate. Now, on mixing the two solutions, a precipitation generally takes place; but this, it is affirmed by certain chemists, is owing to the impurity of the prussiate of pot-ash, and is occasioned by the decomposition of fulphate of pot-ash by muriate of barytes. The truth of this explanation may be ascertained, either by an attentive examination of the precipitate, or by employing a prussiate of pot-ash perfectly free from such impurity.

The former of these tests, as I have stated in your Journal, II. 171, I employed several months ago. Having found that prussiated barytes is soluble in diluted muriatic acid, which exerts no action on the fulphate of that earth, I digested a precipitate, thrown down from muriated barytes by prussiated pot-ash, with diluted muriatic acid. Part of the precipitate was taken up; and the solution betrayed, to the proper tests, decisive characters of its containing prussiated barytes. Since the receipt of Mr. Kirwan's letter, I have repeated this experiment several times; occasionally with the result, which has just now been stated; while, at other times, none of the precipitate was dissolved by the acid. These contradictory results will be explained by what follows; but not being able to devise the cause of them, at the time they occurred, I had recourse to the remaining expedient, viz. the employment of a prussiate of pot-ash, perfectly free from fulphuric salts.

The difficulty of obtaining a prussiate of the required purity in the common mode, is hardly conceivable, except by those who have made the attempt. In your Journal, II. 171, I have asserted that both carbonate and fulphate of pot-ash are decomposed by prussiated barytes; and it has since occurred to me, that an excellent mode of preparing prussiate of pot-ash might be founded on these facts. For the small portion of fulphate of pot-ash, contained in all carbonates, may be expected to be decomposed by the prussiated barytes, provided this last be employed in sufficient quantity. On making the experiment, the result answered my expectation; and I shall, therefore, give the process, which I have since frequently repeated with the same success, under the form of a practical rule.

1. Calcine any quantity of carbonate of barytes in a strong fire, a sufficient length of time to expel its carbonic acid. Dissolve the pure barytes in boiling water, and add by degrees pure Prussian blue in powder, till it ceases to be discoloured. Filter the solution through paper, and if it should become muddy during cooling, owing to the deposit of oxyde of iron, filter it again. After having stood a few hours, small yellowish crystals will appear, which are the prussiated barytes. From the remaining solution a further quantity of crystals may be obtained by evaporation.

2. To a solution of carbonate of pot-ash, gently heated, add by degrees the prussiate of barytes in powder, till the solution no longer restores the colour of reddened litmus paper.

Rather



Rather more of the prussiate of barytes should be employed than is required to decompose the carbonate. After digesting the mixture about half an hour, filtrè the liquor, which, when gently evaporated, will shoot into beautiful crystals of prussiate of pot-ash. These crystals contained, in one instance, 24 per cent. of oxyde of iron; but of this oxyde a considerable part may be separated, by digesting the solution, before evaporating it, with a little acetic acid (radical vinegar.) The acetic has this advantage over all other acids, that its combination with pot-ash affords a salt incapable of crystallizing; and which cannot, therefore, mix with the crystals of prussiated pot-ash.

The carbonate of barytes, which is thus re-generated, may be reserved; and may be again fitted by calcination for the same process.

With the view of deciding the question in dispute, I added to a solution of prussiate of pot-ash prepared, in the above manner, a solution of muriated barytes. No precipitation ensued; nor was the transparency of the mixture in the least degree disturbed. This experiment, therefore, in addition to those before related, convinced me that I had been deceived; and that Messrs. Meyer and Klaproth were correct in denying the precipitation. Happening, however, to examine the jar, about half an hour afterwards, I observed that a number of small crystals had formed on its inner surface; and these, on allowing the mixture to stand a few hours, increased considerably in number. I next mixed the two solutions in larger quantity, with the same result. No immediate effect was apparent; but after a few hours, an abundant crop of small crystals had formed at the bottom and on the sides of the jar. These crystals had the following character:

1. They dissolved very sparingly in cold water, viz. in about the proportion of a quarter of a grain to each ounce.

2. Hot water dissolved them more readily, but still only in very small proportions.

3. From these watery solutions sulphate of pot-ash, or sulphuric acid, precipitated sulphate of barytes; and on adding the sulphate of iron to another portion of the solution, an abundant precipitation of Prussian blue occurred.

4. The crystals were totally dissolved by dilute muriatic acid; and the solution gave decided marks, to the proper tests, of containing prussiated barytes.

5. When heated to redness in a silver vessel, they grew black and lost their form. On adding dilute muriatic acid to this coal, an effervescence ensued, and the solution was muriated barytes.

The foregoing characters are peculiar to the salt termed prussiate of barytes, the formation of which, under the above circumstances, proves, beyond all doubt, that a double elective affinity is exerted between muriate of barytes and prussiate of pot-ash. In this respect, barytes differs from other earths, and approaches the metals.

The above experiments furnished a clue, which led to the explanation of the contradictory properties, observed, at different times, in the precipitate from muriated barytes by prussiate of pot-ash. In trials, which I have since made, it has uniformly happened, that common prussiate of pot-ash has precipitated muriate of barytes. When the  
supernatant



supernatant liquor was decanted immediately, the precipitate, after having been washed, proved to be simply sulphate of barytes; but when the two solutions were allowed to stand together some time, before separating them from the precipitate, this last had constantly the properties of a mixture of sulphate of barytes, with prussiate of barytes; and sometimes crystals of the latter salt were perceivable in the mixture.

Before taking my leave of the subject, allow me to correct an erroneous statement, which I have made in your Journal, II. 171. I have there stated that caustic barytes does not precipitate prussiate of pot-ash. This experiment I have since repeated with pure prussiate described above. No immediate precipitation ensued (the circumstance that before misled me); but after a few hours small crystals were deposited, which had every character of prussiate of barytes. It follows, therefore, that in a table of the elective affinities of Prussian acid, barytes should be placed above pot-ash.

I am, Sir, very respectfully,

your obedient servant,

WILLIAM HENRY.

## VIII.

*On the Genuineness and Purity of Drugs and Medicines. By Mr. FRED. ACCUM.*

(Continued from page 122. Vol. II.)

### EXAMINATION of ALKALINE SUBSTANCES.

*Prepared Kali. Pharm. Lond. Salt of Tartar.*

THE salt of tartar of the shops, if prepared according to the London Pharmacopœia, cannot be obtained pure from pearl-ashes by mere solutions, colature, evaporation, and crystallization; hence this article generally contains a considerable portion of *sulphate* and *muriate of potash*, and is never perfectly free from *siliceous* and *calcareous earth*. In order to discover its purity, one part ought to be dissolved in eight parts of distilled water, and then neutralized with pure nitric acid: if the mixture remain perfectly transparent when cold, the alkali is free from siliceous earth; whereas on the contrary, if siliceous earth be present the mixture will become turbid, the siliceous earth will be separated, and its quantity may thus be ascertained.

Sulphate of pot-ash, the next article generally found in this salt, is best discovered by adding gradually to a similar neutralised solution, a solution of muriate of barytes, or acetite of lead. If sulphuric acid be present, a white precipitate will be produced, but no such effect will take place if no sulphuric combination be present. One hundred parts of the precipitate, occasioned by the admixture of the muriate of barytes, indicate 26 per cent. of sulphuric

phuric acid, and 100 parts of the acetite of lead indicate 30 per cent. of acid. The presence of muriatic acid may be ascertained by adding to a like saturated solution of this salt, a few grains of nitrate of silver. If the alkali did not contain any combination of muriatic acid, it will have no effect upon it; but if the minutest quantity of muriatic acid is present, a white precipitate will be produced, which after some time acquires a bluish hue; neither nitric nor acetic acid will redissolve it. Calcareous earth is manifested in a similar manner, by adding drop by drop, a solution of oxalic acid. The quantity of the precipitate thus obtained, if calcareous earth be present, gives the proportion of this earth in a given quantity of the salt submitted to the experiment.

*Pure salt of tartar*, which stands all these tests, is of a snow whiteness, inodorous, perfectly dry, and soluble without any sediment in an equal quantity of pure cold water, it then contains generally 70 parts of alkali, 23 carbonic acid, and 7 of water.

*Water of Kali. Pham. Lond. Solution of Salt of Tartar.*

When salt of tartar is exposed in a shallow vessel to the action of a moist atmosphere, it soon deliquesces, or attracts so much water, as is sufficient to render it liquid. This fluid ought to contain at least one part of alkali in four. If so, it feels extremely greasy or slippery between the fingers, on account of it dissolving the epidermis, and converting it into a kind of soap. If the alkali made use of has been pure, this fluid is *perfectly colourless*, clear, void of smell, and possesses all the properties of a pure salt of tartar. But as the alkali made use of in general, by our chemists, is far from being so, the article we meet with has generally a yellowish tinge, a disagreeable, somewhat urinous, smell, and soon deposits a considerable quantity of earthy insoluble matter.

*Water of Pure Kali. Ph. L. Solution of Caustic Pot-ash.*

As it is called in our Pharmacopeia, ought to be a solution of caustic alkali in water, of such a strength that an exact pint shall weigh just 16 oz. troy. When well prepared, it is as limpid as water, and inodorous. It ought not to occasion any precipitate with lime water, neither ought it produce the least effervescence with acids. A redundancy of lime is best known by blowing into this fluid, through a tobacco pipe, or other tube. If too much lime be present, the fluid will then turn turbid. Instead of obtaining this article in the shops, we take common soap-leys of the soap manufacturers, diluted with water, hence this article is always yellow, and hence the redundancy of lime, and other heterogeneous salts found in it.

*Prepared Natron. Ph. L. Mineral Alkali.*

As it is no easy task to obtain mineral alkali *perfectly pure*, we need not expect to find it so in commerce. This article, if prepared agreeably to the directions of the Royal College of Physicians, cannot but be contaminated with *sulphate* and *muriate of soda*. But what is still worse, the fraudulent chemists mix sulphate of soda with it intentionally. In order



order to discover the fulphuric and muriate salt, a determinate quantity of the alkali is to be saturated with acetous acid, and then divided into three different glasses. By adding to the first gradually a solution of muriate of barytes, the quantity of fulphuric acid may be discovered, in the same manner as mentioned before. In the second glass, having let fall a few drops of nitrate of silver, the quantity of muriatic acid may be investigated in a similar manner; and by adding gradually a little tartareous acid to the third glass, an acidulous tartrate of pot-ash will be separated in the form of a white powder, 300 parts of which contain 107 per cent. of vegetable alkali, as  $75\frac{1}{2}$  per cent. pure alkali, free from carbonic acid.

The best mineral alkali, free from any heterogeneous admixture, crystallizes in oblong druseated rhomboidal crystals, as clear as glass. It is soluble in an equal quantity of cold water, and generally contains 80 parts of alkali, 64 carbonic acid, and 256 of water.

*Water of Pure Ammonia. Ph. L. Water impregnated with Ammoniac.*

The perfectly caustic spirit of sal ammoniac, as it is usually called, is one of those articles we seldom meet with. It generally contains a considerable portion of carbonic acid, and is also not sufficiently strong (particularly for philosophical purposes). If this liquid is considerably strong, a phial containing exactly 224 grains of distilled water must comprise no more than about 216 grains of aqua ammonia.

*Water of Ammonia. Ph. L. Solution of Carbonate of Pot-ash.*

In order to judge of the relative strength of this article, which is nothing else but a mere solution of carbonate of ammoniac in water; strong ardent spirit may afford no mean criterion of the strength or proper saturation, as well as of the purity of this fluid. If this fluid be fully saturated with carbonate of ammoniac, a quantity of strong ardent spirit, poured slowly down the sides of the glass, will separate the alkaline salt, by uniting with the water, which kept it in solution, in consequence of which an opaque dense coagulum will be formed on the surface where the fluids touch each other; and if on shaking them briskly together, the whole mixture should become converted into a nearly consistent mass, this article is very good. Whereas, on the contrary, if weak, the effect will be far less considerable, and the coagulum will constantly remain separated into two distinct parts, viz. a solid and a fluid one.

Water of ammonia fully saturated with carbonate of ammoniac has the specific gravity 1,150.

*Prepared Ammonia. Ph. L. Carbonate of Ammoniac.*

This salt should be totally volatile if laid on an ignited substance. It is frequently offered for sale by chemists in the form of a coarse powder, which is nothing else than a mixture of muriate of ammoniac and pot-ash; this composition of course is not entirely volatilized by heat. The carbonate of ammoniac imported from abroad always contains vestiges



of iron, and sometimes a considerable quantity of lime. In order to discover the presence of iron, a little of the salt is to be saturated with acetic acid, a few drops of prussiate of pot-ash is then to be added, which will impart to the solution a blue tinge if the minutest quantity of iron be present. The presence of calcareous earth is manifested by means of oxalic acid, occasioning a white precipitate of carbonate of ammoniac; that containing the least quantity of iron has a yellowish tinge, whereas pure carbonate of ammoniac is of a snow whiteness, perfectly dry, somewhat sonorous, volatile before  $212^{\circ}$  Fahr. and soluble in 2 parts of distilled water at  $55^{\circ}$  Fahr. Its specific gravity is generally 1,4076, and its component parts are 40 ammoniac, 53 carbonic acid, and 7 water.

*Salt of Hartshorn. Ph. L. Carbonate of Ammoniac soiled with Animal Oil.*

The genuineness of this article (which differs from the former, merely, in being impregnated with a small quantity of animal oil) may be discovered in a similar manner.

*Solution of the above in an Aqueous Fluid. Volatile Liquor of Hartshorn. Ph. L.*

Is in general imitated by mixing a quantity of aqua ammoniac purae with the officinal spirit of hartshorn, in order to make it stronger, and to give it the appearance of a spirit well saturated with volatile salt. The spirit thus becomes more pungent, so as to bear an addition of a considerable portion of water, without betraying the imposition either by the taste or smell. This fraud may be discovered by adding strong ardent spirit to the sophisticated spirit; for if no considerable quantity of crystallized volatile salt becomes precipitated, the adulteration may be suspected. It may even be discovered by the circumstance of the spirit not effervescing briskly with acids.

Spirit of hartshorn carefully distilled is colourless. Its taste is not very unpleasant (saline not pungent), and its smell somewhat empyreumatic. By long keeping it contracts a yellowish tinge, and acquires a bitter taste and nauseous foetid smell. If fully impregnated with volatile alkali, its specific gravity is about 1,500.

## IX.

*A Memoir on the Ammoniuret of Cobalt, and upon a new Acid contained in the Grey Oxide of that Metal, known by the Name of Zaffre. By LOUIS BRUGNATELLI\*.*

MY attention has been for some time directed to the ammoniacal combinations of the metals, or the ammoniurets. They have hitherto been little examined, excepting that of copper, though their characteristic properties distinguish them very prominently from other

\* Translated from the Italian manuscript of the author, by Van Mons, into French in the annals de Chimie XXXIII, 114, whence the present translation is made.

bodies. The ammoniu ret of cobalt is that to which I directed my first researches \*, and I now send you an account of the peculiarities they have offered.

1. I had several times observed that the precipitate, formed by ammonia in the solution of the nitrate of cobalt, was redissolved in that alkali. I collected this precipitate on a filtre, and washed and dried it. Its colour was dark. Upon half an ounce of this precipitate I poured two ounces of liquid ammonia, after which I closed the bottle, and left the mixture at rest; the temperature of the atmosphere being  $20^{\circ}$  above the zero of Reaumur. At the expiration of 24 hours the alkali had acquired a deep red colour †, and the precipitate was entirely dissolved. I supposed I had formed a perfect ammoniu ret; but nevertheless I endeavoured to procure a larger quantity by other means.

I attempted without effect to dissolve smalt, or the blue (glafs) of cobalt, in caustic ammonia, even with the assistance of long continued digestion; but solution took place without difficulty with the grey oxide of the same metal, commonly called Zaffre. This substance afforded me an easy means of procuring the ammoniu ret in abundance, and to repeat and vary my experiments in several different manners.

3. I evaporated the ammoniu ret obtained from zaffre to dryness. The concrete residue which I obtained was composed of two very distinct substances, one of which was of a deep red colour, and the other of a pale yellowish tinge.

4. I poured distilled water upon this residue, agitating the mixture with a spatula of glafs. The red portion was totally dissolved, and communicated a beautiful rose-colour to the water. The yellowish matter remained undissolved.

5. The yellowish substance may be obtained by evaporating the liquid ammoniu ret, from which it is separated, the moment the fluid is reduced to half its original bulk. The red substance remained dissolved in the last fourth part of the liquid ‡.

6. Ammonia therefore takes from zaffre in its solution two very distinct substances; of which the one soluble in water has a red colour, and the other insoluble in the same liquid is yellowish. This last substance is the pure oxide of cobalt. We think we have discovered a peculiar acid in the former, distinct in its properties from every other known acid.

*Concerning the pure Oxide of Cobalt.*

7. The yellowish substance which is separated by slow evaporation in the air, or in the sun's light, may be considered as the pure oxide of cobalt. In fact it is inspid, inodorous, insoluble in water, and soluble in the mineral acids. The nitro-muriatic acid forms with it

\* I have since examined the ammoniu rets of mercury, zinc, copper, and arsenic. B.

† Bergman observed only that ammonia assumed a red colour with cobalt. "Cobaltum," says he, "a niccolo differt quod omnibus acidis et alcali volatile solvatur colore rubro." This circumstance has been repeated by chemists without addition. B.

‡ A yellow matter is separated from the ammoniu ret of cobalt, if kept for a time, even in well closed bottles. B.



a yellowish solution, which loses most of its colour by the addition of a small portion of distilled water.

8. This solution may be used as a sympathetic ink, like the common muriate of cobalt. Sometimes the acid refuses to dissolve the whole of the oxide; but the solution is completed by the addition of a small quantity of water.

9. This solution is precipitated by the prussiate of pot-ash, of a blueish green, which does not change. It is not precipitated by the gallic acid, but the solution becomes of a deeper colour. In other respects this solution has the same habitudes as the common solution of cobalt in the same acid.

10. The muriatic acid dissolves the yellow oxide very well, and assumes a very deep green colour, which immediately disappears with the nitrous acid, and also by a small portion of water. But it is revived again by the addition of a small quantity of well concentrated muriatic acid.

#### *Of the pure Ammoniuret of Cobalt.*

11. The yellow oxide is totally soluble in ammonia, and forms the pure ammoniuret. Its colour is yellow, and sometimes rose coloured; the acids do not decompose it; the muriatic acid discolours it: prussiate of pot-ash gives it a grey colour, and afterwards throws down a precipitate of the same colour. The sulphuret of pot-ash causes it to assume a deep colour, inclining to black, and throws down sulphuret of cobalt\*.

#### *Concerning the Acid obtained from the Ammoniuret of Cobalt, and its Properties.*

12. The red substance of the preceding experiments was separated from the yellow oxide. For this purpose I evaporated the liquid ammoniuret in the sun, and when it was reduced to about one-fourth of its volume, and no longer afforded any yellow precipitate, I filtered it through paper. The fluid was of a deep red colour, like that of cochineal. This liquid emitted no smell, but its taste was very sharp. It was exposed to the sun's light till perfectly dry.

13. The remaining mass was dissolved in distilled water, which acquired a fine ruby colour. This solution exhibited unequivocal signs of acidity. It deposited on cooling some small brilliant crystals, which I found to be a combination of the acid with ammonia. This acid appearing to be distinct from every other, I have thought fit to call it the *cobaltic acid*.

In order to ascertain whether heat was capable of raising the cobaltic acid, I submitted one pound of the ammoniuret of cobalt to distillation in a retort. When three-fourths of the liquid had passed into the receiver, I stopped the distillation. The retort contained

\* The sulphuret of cobalt, dried in the air, resembles zaffre in its colour. When rubbed on paper, it assumes the metallic brilliancy, as do most of the other metallic sulphurets. It emits a sulphureous smell when heated, and takes fire when thrown on ignited coals. B.



a liquid rendered turbid by the precipitated yellow oxide. I decanted the liquid after the precipitate had settled, and evaporated it to dryness. The residue was yellowish, and partly soluble in water, to which it communicated a yellow tinge. This solution exhibited all the acid characters of the red liquid, No. 15.

15. In another experiment, I put one pound of the ammoniuret of cobalt into a retort, and urged the distillation to dryness. The fixed matter which remained on the bottom of the retort was blue at the surface, but internally yellow. After some hours the blue colour totally disappeared, and was changed into red.

16. A remarkable difference between the residue of the evaporation by the sun's light, and that by the fire, was that the latter transmits the cobaltic acid nearly colourless; so that the cold solution is nearly as limpid as water. I have besides remarked, that by this last process the acid contains little or no cobalt of ammonia.

17. I shall here present the principal characters which distinguish this new acid. They are

1. Its form is concrete, and it is not volatilized by fire.
2. In some instances it is red (13); in some pale yellow (14); and in some colourless (16).
3. It is without smell.
4. It has a sharp, and not unpleasing taste.
5. It strongly reddens the tincture of turnsole.
6. It is perfectly soluble in water.
7. It decomposes all the fulphurets of alkali, and precipitates the sulphur.
8. It precipitates the ammoniuret of copper of a light green, and that of zinc of a clear white.
9. It precipitates the fulphuret of copper of the same colour as the ammoniuret of that metal.
10. It precipitates the nitrate of silver white.
11. The nitro muriate of tin the same.
12. The nitrate of mercury of a light straw colour.
13. The acetate of lead white.
14. It does not sensibly affect the solutions of gold and platina.
15. It precipitates lime water in a white coagulum, insoluble in water, and in excess of acid.
16. It precipitates the acetates and muriates of barytes.
17. It is separable by alcohol from its solution in water.
18. When employed as a sympathetic ink, it does not give a green or blue colour, like the solutions of cobalt; but gives a brown and afterwards a black colour to paper when rather strongly heated, as happens with other acids.
19. It affords, with new made tincture of galls, a yellow abundant precipitate.

20. With

- 20. With the saturated solution of soda, it affords an irregular transparent salt soluble in water, and not deliquescent.
- 21. With pot-ash it affords a crystallizable salt in square crystals, which are transparent and permanent in the air.
- 22. With ammoniac it forms a salt soluble in its acid.
- 23. With barytes it forms an opaque salt, crystallizable with difficulty.

*Supplement.*

The presence of an acid in zaffre, induced me to suspect that it might probably be arsenical: but this doubt soon vanished, by comparing the characters of each.

- 1. The arsenical acid does not precipitate the solutions of silver, as does the cobaltic acid.
- 2. The arsenical acid precipitates lime water; this arseniate is re-dissolved by the acid, as well as by a new quantity of lime water. The contrary happens with the acid of cobalt.
- 3. The arsenical acid does not decompose the muriate and acetate of barytes, as does the cobaltic acid.
- 4. The arsenical acid is soluble in alcohol, which precipitates the cobaltic in a concrete form.

It remained to be proved, whether the acid obtained from zaffre existed ready formed in that oxide, or whether it was produced by the action of the ammonia.

As the acid of cobalt is very soluble in water, I boiled six pounds of zaffre in eight pounds of water for a quarter of an hour, and filtered the liquid while yet warm. That which passed was transparent and colourless; but had a perceptible taste. I evaporated the liquid, taking care to cover the vessel with a piece of silk. When it was reduced to one half, it became turbid, but the matter which was separated was not perceptibly coloured. I continued the evaporation till no more than one-third of the liquid remained, and then removed it from the fire. It deposited a very white matter, which the contact of the air changed to a beautiful rose colour. I separated this matter, and collected it on the filtre.

The filtered fluid was of a bright yellow colour, and perfectly transparent. It had a very decided acid taste; reddened the tincture of turnsole, speedily decomposed lime water, the salts of barytes and of silver, was precipitated by alcohol, &c. In a word, its habitudes were in every respect the same as those of the cobaltic acid obtained by the foregoing processes.

The red deposition which remained on the filtre had no taste, and coloured the muriatic acid of a very fine green. It was the pure oxide of cobalt. This oxide is largely soluble in its own acid, and is precipitated in proportion as the acid is concentrated.

The acid which the ammonia separated from the zaffre, exists therefore ready formed in that substance. Its radical still remains to be positively ascertained. In the mean time I have thought it proper to designate it by the name of the *Cobaltic Acid*.



## X.

*The Apparatus of Woulfe improved, so as not to require luting. By Cit. GIRARD.\**

AS the ingenious apparatus of Woulfe, which has been so highly useful in modern chemistry, always requires a considerable time for its arrangement in those operations in which it is used, the attention of philosophers has been long directed to the means of simplifying it. Various forms have been contrived to construct this apparatus, hermetically closed, without the assistance of luting; but they are in general too difficult in the execution. That which I now offer to the Institute, may be put together or dismounted in a few seconds, and appears applicable in all the circumstances of experiment.

It consists simply in causing the vessels to be made at the glass house, with the neck A, to receive the interior tube A B (Pl. II. Fig. 2.) which is welded in the neck itself by means of a projection in the side of the glass. The tube on the opposite side is first drawn to a point, or thin tube as usual; but then instead of breaking this tube near the bottle, to make it agree with the subsequent fittings, it is left entire and bended, so that it terminates in a curve, which may be introduced into the tube A B. It is unnecessary to remark, that the tube A B, and the neck into which it enters, should have the same curvature, which is very easily effected by bending both on the same cylinder of earth or wood.

It may be easily understood, that the neck of the first bottle being introduced into the tube of the second, beyond which it projects, and both tubes entirely immersed in the water, no gas can escape from the first vessel, but by passing through its neck, and through the liquid it contains.

Fig. 3. represents another apparatus of Woulfe's, which will supply the place of the other in case of distance from a glass house. It is composed of large necked bottles and tubes. Take a somewhat thick tube, of sufficient bore to receive another. The upper part of this must be enlarged by heating and blowing, and it may afterwards be ground and made to fit in the form of a stopper.

Another tube should be then taken, which can enter the first so loosely that it can be moved with ease. The extremity A of this tube is then ground in the same manner, in order that it may fit another bottle, after which the usual curve A B C is given to it.

Lastly, it is introduced into the tube E F, fig. 4. and the prominent part D is bended upwards. The vessel may then be considered as hermetically closed, as soon as the lower aperture of the large tube is immersed in the included water.

The curve of the tubes and necks of the bottles is only intended to direct the bubbles of gas which escape through the neck, that they may not again enter the tube out of which they are required to pass. This curvature is not required, except in small apparatus.

\* *Annales de Chimie*, xxxii. 283.



When a projection of an inch, or more, can be given to the neck beyond the lower opening of the tube, the bubbles must necessarily escape without that precaution.

Fig 5. exhibits another construction of the same apparatus.

If an apparatus, composed of a very long series of bottles were employed, it might be feared, that the gas contained in the first vessel having a very strong resistance to conquer, would cause the fluid of the first bottle to expand and rise in the tube; but it is very easy to remedy this inconvenience, either by using bottles of different sizes, or by putting a smaller quantity of fluid in the first bottles, than in the following ones.

## XI.

*A Memoir on the Discovery of certain Bones of a Quadruped of the Clawed Kind in the Western Parts of Virginia. By THOMAS JEFFERSON, Esq.*

IN a letter of July 3d, I informed our late most worthy president, that some bones of a very large animal of the clawed kind had been recently discovered within this state, and promised a communication on the subject as soon as we could recover what were still recoverable of them. It is well known that the substratum of the country beyond the Blue Ridge is a limestone, abounding with large caverns, the earthy floors of which are highly impregnated with nitre; and that the inhabitants are in the habit of extracting the nitre from them. In digging the floor of one of these caves, belonging to Frederic Cromer in the county of Greenbriar, the labourers at the depth of two or three feet, came to some bones, the size and form of which bespoke an animal unknown to them. The nitrous impregnation of the earth, together with a small degree of petrification, had probably been the means of their preservation. The importance of the discovery was not known to those who made it, yet it excited conversation in the neighbourhood, and led persons of vague curiosity to seek and take away the bones. It was fortunate for science that one of its zealous and well informed friends, Colonel John Stewart of that neighbourhood, heard of the discovery, and, sensible from their description, that they were of an animal not known, took measures without delay for saving those which still remained. He was kind enough to inform me of the incident, and to forward me the bones from time to time as they were recovered. To these I was enabled accidentally to add some others by the kindness of a Mr. Hopkins of New York, who had visited the cave. These bones are,

1st. A small fragment of the femur or thigh bone; being in fact only its lower extremity, separated from the main bone at its epiphysis, so as to give us only the two condyles, but these are nearly entire.

\* American Transactions, IV. 246.

2d. A radius, perfect.

3d. An ulna, or fore-arm, perfect, except that it is broken in two.

4th. Three claws, and half a dozen other bones of the foot; but whether of a fore or hinder foot, is not evident.

About a foot in length of the residue of the femur was found, it was split through the middle, and in that state was used as a support for one of the salt petre vats; this piece was afterwards lost, but its measures had been first taken, as will be stated hereafter.

*(To be continued.)*

*SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.*

*Letter from Dr. Beddoes; with a Proposal for a Course of Lectures.*

TO MR. NICHOLSON.

SIR,

IF you approve the spirit of the undertaking, announced in the enclosed paper, pray notice it in your Journal. I hope the example will be followed. Whether the number of persons, aware of the paramount importance of the science of animal nature, is at present sufficient to ensure a reasonable compensation to those who may attempt to explain it in lectures, I am not uncertain. But this æra is fast approaching. I am persuaded we shall see institutions for this purpose, similar to those which have long existed, or been lately formed for important, but very inferior purposes.

The success at first experienced in the treatment of palsy by the gas, before spoken of in your Journal, continues. I repeat my undoubting conviction, that the introduction of factitious air into medicine, will amply recompence the pains that have been taken to bring it about. I had engaged an able dissector and draughtsman, for the purpose of co-operating with Mr. Davy and myself in physiological researches. But an opportunity offering for his advantage, I of course relinquished my claims upon him; I am much in want of a successor, and I wish some ingenious young man, accustomed to manage the scalpel and the pencil, may be induced, by this notification, to join us in an enterprize which must be congenial to the feelings of every ardent cultivator of medical philosophy. I should make the conditions as agreeable to him as it lies in my power to do.

I am, Sir,

Your's, with great esteem,

March 2, 1800.

THOMAS BEDDOES:

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*Lectures*



*Lectures on the Laws of Animal Nature, and on the Means of preserving the System from Injury upon the most important Occasions of common Life.*

At some convenient place in *Bristol*, Dr. Beddoes proposes to attempt a popular exposition of the principles of the animal oeconomy, with their application to the purposes of individual and domestic welfare, upon a plan widely different from that of any existing publication. For his opinion on the advantage of disseminating physiological information, he may refer to his *Lecture introductory* to Messrs. Bowles & Smith's Course of Anatomy; and an exemplification of the manner in which he thinks the subject ought to be treated will be found in his *Essay on Consumption*.

Heretofore an acquaintance with the causes of his personal condition has seldom been numbered among the *accomplishments* of the scholar, or the *qualifications* with which the man of business is fitted out for success in the world. Yet it will be confessed, that neither success in business, nor proficiency in the sciences, accounted *liberal*, are separately sufficient for rendering the condition of human life desirable. And, in fact, to endeavour by any combination of these materials, to construct a system of personal happiness, is to project an edifice which shall stand secure without a foundation.—Of a truth, so long and so generally neglected, a portion of the public, it is believed, begins to feel that degree of conviction which operates upon conduct. In this belief, the present opportunity of instruction is offered to those who may be desirous of it.

If it be allowed that the moral and physical attributes of human nature are inseparable, persons interested in the art of education will scarce require to be reminded of the value of that species of knowledge which the lectures, here announced, are intended to communicate.

They ought to prevent many of those mortal bruises which travellers along the road of life give themselves for want of knowing the quality and position of the objects in their way.

By presenting a just estimate of *that art* to the operations of which almost every one is sooner or later doomed to submit, they should afford some protection against gross medical incapacity or fraud.

They should reduce to their just value many of those axioms that wander about the world concerning what is wholesome or unwholesome in diet or exercise:—axioms which the instinct of self-preservation impels men to take up; and upon which, however loosely adopted, they act with as full assurance as if they knew them to have the most solid foundation in physiological science.

Numbers fall victims to their own impatience under illness, or to the wavering conduct of their friends. Frequently in the onset of dangerous diseases, people by suffering themselves to be amused by trifling domestic expedients, lose an opportunity which no medical skill can ever retrieve. Upon these evils the prevalence of juster ideas would act as a check. Nor is it paradoxical to suppose that the mortality among infants would be smaller, and debility of constitution at all periods of life more rare, if parents (however instructed in other



other things) were not in common nearly upon a level with nurses in that which it so much imports them to possess—an acquaintance with the powers that operate to the injury or advantage, the destruction or preservation, of the objects of their affection.

The Author further hopes (if he may repeat his own words) to contribute towards preventing the “ignorant from tampering with the sick; towards promoting the ascendancy “of science over intrigue; alluring curiosity from the pernicious frivolities of literature, “and elevating the conceptions of men to the level of their highest interests.”

As the whole course will be connected, the tickets will not be transferable—The number of lectures cannot be determined beforehand—But that there may be little chance of exclusion by reason of narrow circumstances, the subscription is fixed at ONE GUINEA—The lectures will be calculated for both sexes and different ages—They will be delivered in the evening, and commence sometime in April next—probably near the middle of the month—provided fifty persons shall have entered their names by the 31st of March. This condition is indispensable. Without a tolerably numerous audience, the author presumes he could bestow his time in a manner more advantageous to the public.

Subscriptions received by Mr. SHEPPARD, Bookseller, *opposite the Exchange*, with whom conditions for printing a SYLLABUS may be seen.

RODNEY-PLACE, CLIFTON, *March 3, 1800.*

*Extract of a Letter from M. Fabbroni of Florence, to Cit. Van Mons, on the Ethiops of Iron, the Formation of Alcohol and Fermentation. (An. de Chim. XXX. 220.)*

Take a pound of iron filings, reduce them to a paste with water, and put it in a capsule, or what is still better, a glass matras, kept in a water bath at about 50 or 60 degrees (Reaumur). Pour on it gradually one or two ounces of aqua fortis, rather diluted, or of nitrous acid very much diluted, and continue stirring it with a spatula. It is remarkable, that it undergoes a kind of effervescence, after which the iron is changed into a very fine black powder, and oxidized to the first degree of oxidation; that is to say, converted into martial ethiops. The operation is performed in less than half an hour. If the mixture be put in a closed vessel, and not shaken, but left from evening till the next day, the surface of the ethiops is found to be covered with a kind of champignons extremely white, and several lines high, which are nothing but volatile alkali, or carbonate of ammoniac. The air in the vessel is in this last case, composed in a large portion of oxygenated nitrous gas. A decomposition here takes place of the water and nitrous acid, both at the same time, by the iron which seizes their oxygen, in order to become converted into the oxide; and the component parts of these two liquids, that is, the azote and hydrogen, which being disengaged at the same time, meet whilst they are yet in a state of condensation, or before they have taken the form of a gas, by combining are transformed into ammoniac. A portion

tion of oxygen appears also to be precipitated on the carbon of the iron, which affords the carbonic acid, and crystallises the ammoniac, by saturating it as it is formed.

Chemists in general, have believed and advanced; that alcohol is the produce of the vinous fermentation. I am assured that it is not all so, for it does not exist in wine. I think I can render this very evident, if I have a method by which I can recover an hundredth part of alcohol, when completely mixed with strong wine, in which, by the same method, I could not before discover the smallest atom, although I could obtain 20 or 25 of brandy, from a hundred parts by distillation. These were the simple means by which it was performed: take a glass tube of sufficient size to introduce the finger, and divide it into an hundred equal parts; take fresh wine, with which you have mixed one hundredth part of alcohol, and to that put as much pot-ash in powder, as is found by a previous experiment, to be necessary to precipitate the resinous coloring residue. Then strain the wine, and pour it into the tube, and add as much pot-ash in powder as is required to saturate it. The centime of alcohol, which had been mixed with the wine, will then be seen to rise, and swim distinctly on the surface of the alkaline solution. It will be found of the same degree of strength, and in the same proportion that it had been added, if the operation is performed with sufficient quickness to avoid losing any thing by evaporation. The previous separation of the coloring matter serves merely to render the result more perceptible. Now, if by this means I procure only the same quantity of alcohol from the wine that I knew it contained before, and not a drop more, it appears to me, I have a right to conclude, that that which I obtain from the same wine by distillation, did not exist in it before, but that it is the operation of distillation which forms it; that its formation is determined by the heat; and lastly, that this liquid is a product, and not an educt of the distillation of the wine. The heat necessary to effect this is not therefore very considerable, for it is formed by the heat of the fermentation, to an heat of distillation of 14 degrees, such as may be obtained during winter. It may be formed in bottles by the warmth of the atmosphere, &c. It is for this reason, that I have recommended new wine to be used in my experiment. All this has been printed in my *Arte di fare il vino*, which was published at Florence in 1788; but Italian books seldom pass the Alps. The anatomical examination of the grape precedes the chemical part in this work. The theory of fermentation is there found explained according to the ancient principles, though at that epocha I had, for a considerable time, renounced the phlogistic theory. The essential ingredients, the active and passive principles, that is to say, of fermentation, are there deduced from experiment; and the making of artificial wine is the confirmation of them. The work is concluded, by exposing the chemical means necessary to discover the alteration which adulterated wines undergo, and a summary of all the theories of fermentation that have been presented, to the time of its publication. You will there, perhaps, find a phenomenon which contradicts them all, and which is very remarkable. It is that the access of air is not essential to fermentation; for I have excited it in the vacuum of Toricelli.

*Principles*



*Principles of modern Chemistry, systematically arranged, by Dr. Frederic Charles Gren, late Professor at Halle, in Saxony. Translated from the German, with Notes and Additions concerning late Discoveries, by the Translator, and some necessary Tables. Illustrated by Seven Plates. In two Volumes, Octavo, 964 Pages. Price 16s. Cadell and Davies.*

The character of the late Dr. Gren as a chemical philosopher is well known, as is likewise the value of the work from which the present translation is made. I shall, therefore only give a short account of the plan: After a concise historical introduction, the author enters upon his subject, which he divides into twelve chapters, each of which is again subdivided into heads or titles, and those into numerous paragraphs or sections. The first treats of preliminary or general objects, and the operations and instruments of chemistry. 2. The simple substances and process of combustion. 3. Salts in general. 4. Earths. 5. Mineral Acids. 6. Constituent parts of vegetables. 7. Of animal substances. 8. Spontaneous changes in the mixture of organic bodies, fermentation, &c. 9. Bitumens. 10. Mineral coal. 11. Metals. To these the translator, besides many useful notes and additions through the work, has added an appendix, containing the doctrine and tables of chemical attraction, the chemical characters of Hassenfratz and Adet, specific gravities, comparison of thermometers, of weights and measures, together with an index to the whole.

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*The Chemical Pocket Book, or Memoranda Chemica, arranged in a Compendium of Chemistry, according to the latest Discoveries, with Bergman's Table of single elective Attractions, as improved by Dr. G. Pearson. Calculated as well as for the occasional Reference of the professional Student as to supply others with a general Knowledge of Chemistry. By James Parkinson. Small Twelves, 229 Pages. Price 5s. Symonds.*

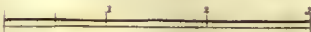
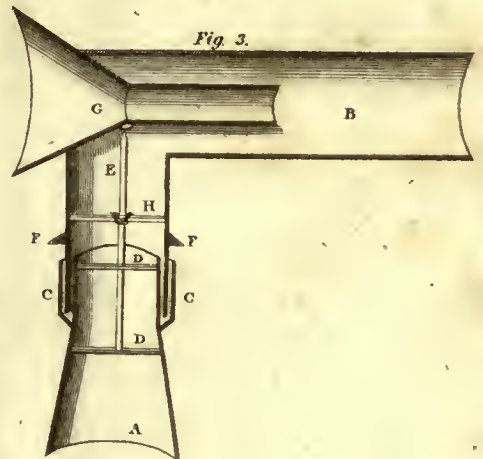
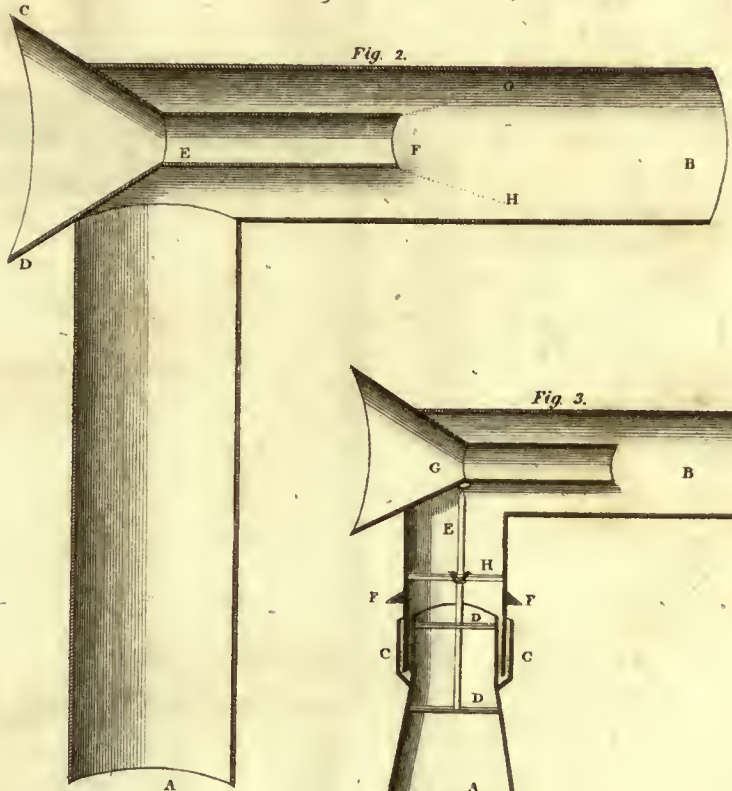
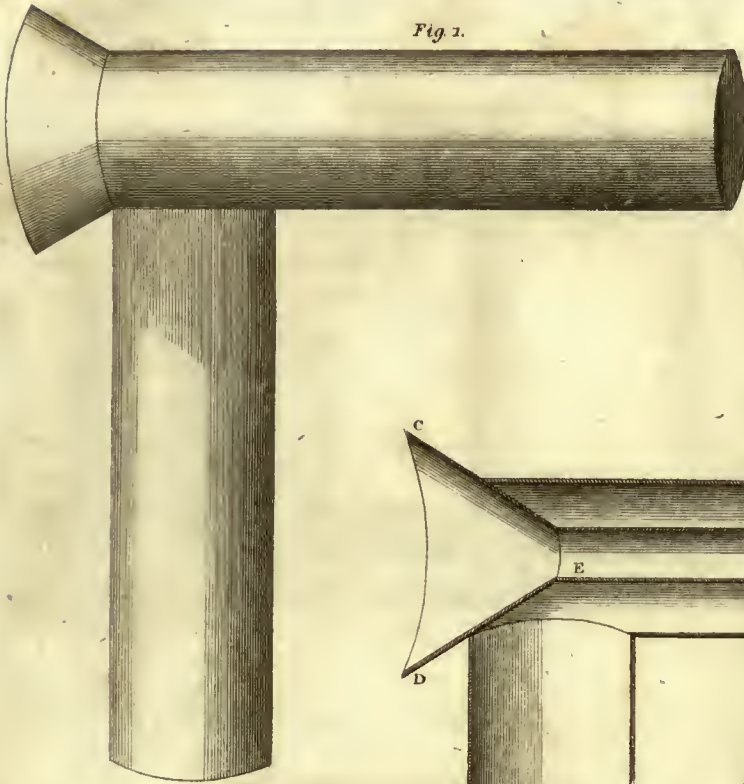
This is a very good compendium. By using the small type, called bourgeois, the author has contrived to give nearly the same quantity of matter in the pages of his little pocket volume as is usually contained in an octavo page. The greater merit, however, consists in the industry he has exerted in collecting the numerous particulars of modern chemistry from the most authentic sources, and condensing them with a degree of neatness and perspicuity which will render his book equally useful as a manual of science to the philosopher and the student. He quotes his authorities much more frequently than is the modern custom; but it will greatly add to the value of his book if in a future edition he should refer in all places by the real title and page to every author of eminence to whom he might wish to refer his reader.



*Royal Institution of Great Britain:*

A course of philosophical and chemical lectures is at present delivered at the house of the Royal Institution by Dr. Garnett, which are received with the most marked attention by an audience of the first respectability. The ordinances, bye laws, and regulations of this establishment have likewise been printed. They consist, for the most part, of a development and organization of the subjects contained in their prospectus of last year. The managers are charged to erect a laboratory and theatre for lectures, and to appoint professors. They elect all the subscribers. Proprietors are to be proposed as candidates by a manager, and elected at the subsequent monthly meeting by a majority of two-thirds, and the sum to be paid will be 60*l.* after May, 1800;—70*l.* after May, 1801;—80*l.* after May, 1802;—90*l.* after May, 1803; and 100*l.* after May, 1804. Subscribers for life, or annual, must be proposed by a manager, and elected at the subsequent weekly meeting. Ladies for these two classes are admitted by recommendation of certain ladies intrusted by the managers to hold books for that purpose. I do not find any specific mention of the sums to be paid by life and annual subscribers; but upon enquiry at the house, I am informed, that on Friday the 7th of this month, the sum for the former class was raised from ten to twenty guineas, and for the latter from two to three guineas. Whether the managers of this great establishment have shewn a *laudable* partiality to their own class by giving them five regular notices of the advances they are to pay, while the the rest of the public is called upon without ceremony for augmentation of fifty and one hundred per cent. may perhaps be questioned.

*Mr. Boswell's Blast Ventilator.*

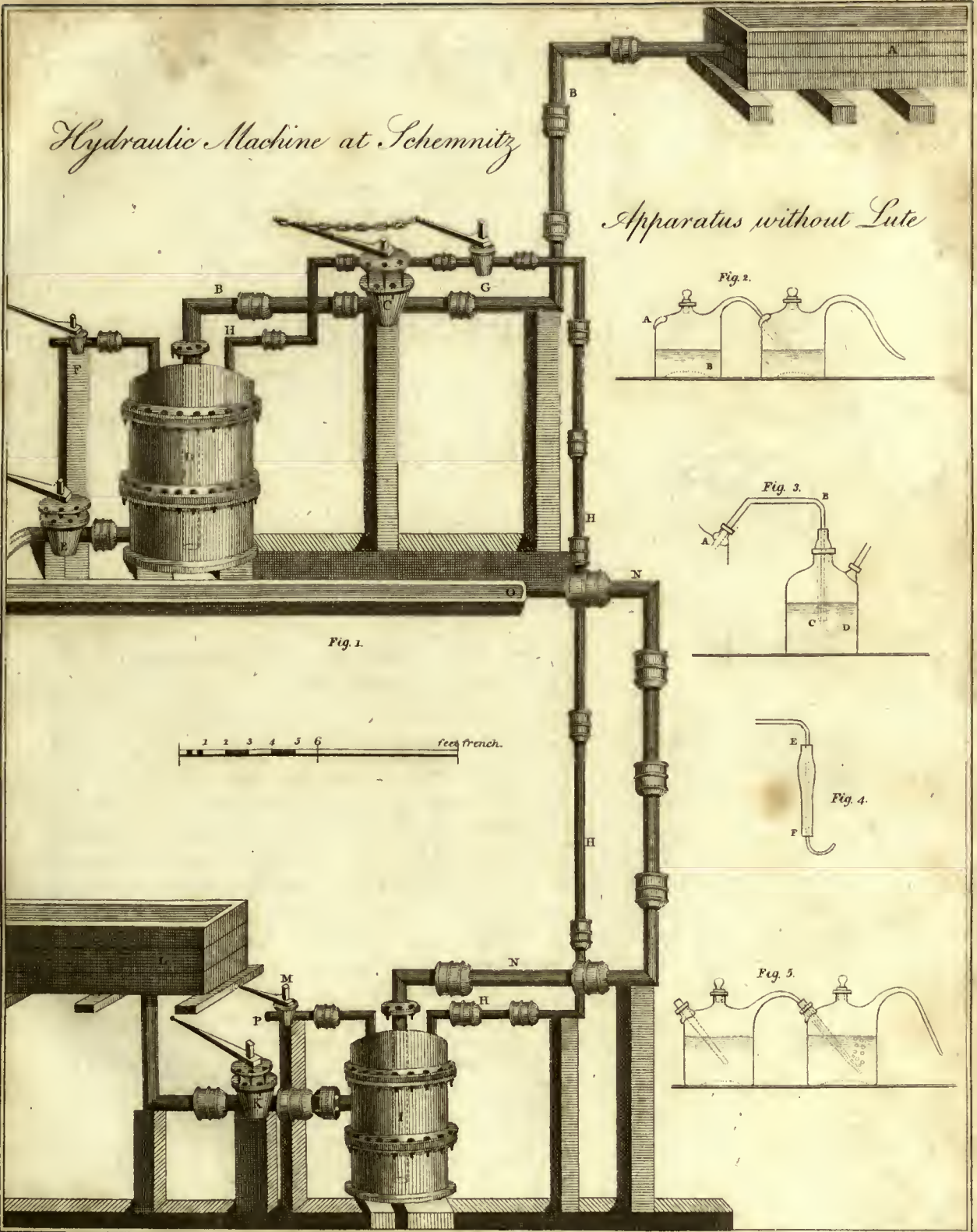






# *Hydraulic Machine at Schemnitz*

## *Apparatus without Lute*





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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MAY 1800.

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ARTICLE I.

*On the different Effects of the Alkalis in the Production of Alum. By Professor  
HILDEBRANDT, of Erlangen\*.*

IT is a known fact, in the usual preparation or extraction of alum, that pot-ash, or urine, is added to the first solution, which is procured by lixiviating the roasted and weathered aluminous ores. *Bergman* †, being of opinion that these additions tended only to saturate the superabundant acid of the lye by which the crystallization of the salt is impeded, has advised to add pure aluminous earth, or argil, instead of the preceding substances, on the supposition that those additions do contaminate the alum with foreign salts; while, on the contrary, pure alumine would not only saturate the superfluous acid, but increase the quantity of the sulphate of alumine. *Professor Lampadius* ‡, who adopted the same opinion, tried by experiments, according to *Bergman's* idea, whether he could not render

\* Translated from Dr. Scherer's (now Professor at Halle in Saxony) Chemical Journal, Vol. II. pag. 419.

† *Bergman, de confectione aluminis*, § II. *Opusc. Chem. et Phys. I. Lips.* 1783. 8vo. pag. 308. Quum autem argilla pura basin aluminis constituat, excedens lixivii acidum nullo alio modo melius, quam ea donatur, quæ superfluum acidum non tantum aufert, sed etiam salis quæsitæ copiam auget.

‡ *Lampadius, pract. chemische Abhandlungen I. Dresden 1795*, § 14.



the addition of pot-ash or urine unnecessary, and with this view he had, in the alum-works of Lederbur at Weisgrun, added to the crude lye some white argil, dug up near the village Chotta.

The superabundant acid here spoken of, is not that portion which is essential to the alum, the acid in this salt being never fully saturated with argil; but that quantity only is meant, which may happen to be present in the lye as foreign, or not belonging to the composition of alum, as is the case, when the aluminous ore is too abundant in sulphur, or when a sufficient portion of its argillaceous part has not been combined with the sulphuric acid generated in the process.

If we admit that the crude lixivium of alum does really contain an excess of acid, and that it is this only which impedes the crystallization of the salt, the addition of argil seems much more proper, than that of either pot-ash or urine; because more alum must be obtained by the former, while the latter affords sulphate of pot-ash or ammoniac, which diminish the purity of the product.

But *Marggraf*\* has long ago observed, that the sulphate of alumine cannot be generated from argil and sulphuric acid alone, and that an alkaline salt is one of its essential ingredients. To prove this fact, I have several times dissolved some aluminous earth (that had been precipitated from Roman alum by means of pot-ash, and properlyedulcorated) in sulphuric acid carefully rectified, and attempted to crystallize the clear filtered lye, by evaporation and cooling. But I never obtained the least appearance of crystallized alum. The mixture consisting of argil and sulphuric acid, rather in excess, is more soluble in water than true alum: so that two parts of water at a temperature of 50° Fahrenheit, are sufficient to dissolve one part of that mass, merely dried without ignition; whereas the alum requires at the same temperature, 35, or more parts of water, to dissolve it. Even after that lye had been reduced by evaporation to a much smaller compass, than a lixivium of true alum would admit of without crystallizing, it continued liquid, though exposed to a freezing cold. On evaporating it still farther, and by subsequent cooling, it affords indeed a compact mass, but without any determinate figure, which occupies the bottom of the vessel in the form of a dried jelly. It was but seldom that, in a certain degree of the evaporation, I could perceive a kind of crystallization in groups; but there was no appearance of such hard octahedral crystals, as present themselves in a solution of true alum, containing upwards of twenty parts of water to one of salt dissolved in it †.

If, on the contrary, to the above solution of argil in sulphuric acid, a small quantity of vegetable alkali only be added, and it be then evaporated and left to cool, regular crystals of alum will be obtained at each subsequent refrigeration.

\* *Marggraf*, Chim. Schriften. I. Berlin, 1768. S. 196.

† *Marggraf*, however, observed that when the argil was strongly calcined, it afforded some crystals with the sulphuric acid, which resembled those of alum, but were not so fine as those obtained by the addition of an alkaline lye. I wish he had stated this more accurately. But, as my present object was to ascertain, which of the alkalis may be most suitable to the generation of alum, I have not yet repeated that experiment.

In order to discover the proportion of the alumine to the sulphuric acid, nothing more is required, than to combine as much of the earth with the acid, as it will dissolve in the common temperature (from 50° to 60° Fahrenheit). An excess of the acid will, in that case always remain, as shewn by the test of Litmus. For at that temperature the acid does not take up so much of the argil as to become neutralized. The solution must, however, be left standing long enough (24 hours) upon the remaining undissolved part of the earth, because the argil is dissolved much more slowly by this acid, than the calcareous and magnesian earths.

In order to discover the due proportion of pot-ash in this process, *Marggraf* directs us to continue adding gradually of the alkaline lye by drops, till a light pulverulent precipitate (that is to say, precipitated argil) appears. In the following experiments I added so much alkali, whether pot-ash, or either of the other two, that (the quantity of dissolved pure alumine being half an ounce) there remained a sufficiently perceptible quantity of precipitated earth, though very small in proportion to the whole. For, it is to be observed, that the very first drops of the alkaline lixivium, if neither itself nor the solution of the earth be too weak, produce a turbidness; but this first precipitate is re-dissolved in the excess of the acid present. I am much inclined to suppose, that it may be immaterial to determine the precise quantity of alkali, because I have constantly obtained alum by the addition of pot-ash, notwithstanding that in some instances I added more of it, and in others less.

To ascertain the regular form of the crystals of alum, one or more threads should be hung into the lye, in order that perfect, or nearly perfect octahedrons may be formed round them; for those crystals which are formed at the bottom, or on the sides of the vessel, are always defective in one or more of their angles.

Having decided from these experiments, that a small portion of alkali is essentially requisite to the production of sulphate of alumine, I mentioned the fact in the advertisement of *Prof. Lampadius's Chemical Essays*. It cannot be denied, but that in one of his experiments \* he has obtained alum, from mere argil and sulphuric acid, without the addition of either pot-ash or urine: but not to mention, that the quantity of alum obtained by the said process was indeed exceeding small; in proportion to what has been produced on adding pot-ash or urine, it is probable, that as much vegetable alkali as was sufficient for the formation of that small portion of alum may have entered into the crude lixivium, from the ashes of the wood employed in the roasting of the aluminous ore, and of which a greater or less portion is unavoidably mixed, with the roasted mineral. Or that alkali may actually have pre-existed in the ore; (or if not, at least the nitrogen and hydrogen, from which the alkali was generated, may have been present.) *Profess. Klaproth* † has discovered pot-ash in

\* *Lampadius*, loc. cit. pag. 16.

† *Klaproth* discovered in the native alum from *Miseno* so large a proportion of pot-ash, that 1000 grains of it afforded 470 grains of crystallized alum, and in these 27 grains of sulphate of pot-ash. (*Beytrage zur Kenntniss der Mineral Koerper*. I. Berlin, 1795. theile 313.)



the *native alum* from *Miseno*. Only one part of its aqueous solution would crystallize into genuine alum, in which also he traced out the alkali; the remaining portion of the lye yielded no alum spontaneously, but only when a little pot-ash had been superadded. *Bergman*\* likewise, found that alkali in the artificial alum, even in the Roman; and Prof. *Lampadius*† himself considers it at present as an essential-principle of alum, in consequence of having found it in four different species of factitious alum: namely, those from *Commin* and *Weissgrün* in Bohemia, and those from *Reichenbach*, and from the *Schwemfal* in Saxony.

On considering the share, which the vegetable alkali has in the production of alum, the question naturally arises: whether *pot-ash* alone, or *soda* likewise, as well as *ammoniac*, are capable of combining with sulphuric acid and argillaceous earth into alum ‡?

The aptitude of volatile alkali to enter into such union, seems to be deducible from the circumstance, that, in some alum-works, urine only and no pot-ash is employed. However, as I have already observed in another respect, it is not impossible but that a quantity of vegetable alkali may be afforded by ashes formed in the roasting of the alum-ore, sufficient to complete the alum.

The alkaline portion of the alum may be ascertained in three different ways by analysis.

1. Let the argil be thrown down from a watery solution of alum by means of *ammoniac*; cause then the salt contained in the clear decanted lixivium to crystallize, and subsequent to this expose it to a subliming heat. In this process, the *sulphate of ammoniac* sublimes, but the *fixed sulphate of pot-ash*, if any has been in the alum, remains behind it in a compact form. This method discovers the presence of fixed alkali only.

2. To a boiling hot aqueous solution of alum, add carbonate of lime. By this management the sulphate of argil is decomposed, and the lime is converted into gypsum; at the same time that the sulphate of pot-ash, if any was present, remains unaltered. This last would likewise be decomposed by caustic or pure lime; but it is well known that the crude calcareous earth, on account of its strong attraction for carbonic acid, has no such power on sulphate of pot-ash. What quantity of crude lime may be necessary to accomplish that decomposition, may in some manner be determined, by the known proportions of argil to sulphuric acid in alum, as well as by the proportion of calcareous earth to the same acid in gypsum, and lastly that of crude lime to carbonic acid in carbonate of lime. However, as this determination is subject to some difficulties, it will be sufficient to be attentive to the effervescence of the lime, and according to this effect, to add rather too much than too little, because the redundant calcareous earth falls down, unchanged, together with the

\* *Bergman*, cit. § 77, page 307. Yet he says: *hanc raro*.

† *Lampadius*, *Pract. Chim. Abhandlungen* II. 1797. S. 228.

‡ *Marggraf* (cit. loc. page 196) says only, that the addition of fixed alkali is necessary.---*Lampadius* (cit. p. 230) has not yet given any results of his experiments, made with the mineral and volatile alkali.---*Bergman* (cit. 307.) says: *crystallizationis obstaculum alcali volatili æque tollitur, non vero alcali minerali et calce*; yet without relating any experiment.

precipitated.



precipitated argillaceous earth, from which it may be separated by the known methods, if it should be required to ascertain the quantity of the argil. A certain portion of the sulphate of lime, thus produced, is likewise precipitated at the same time, and in greater quantity, the less water there is in the solution. The remainder of the gypsum continues dissolved in that fluid. Let the liquor be carefully poured off from the precipitate, and let this last beedulcorated with a sufficient quantity of boiling water; let the washings be added to the fluid at first decanted; and the whole of the fluid be gradually evaporated, and left to cool, by alternations. By this management the gypsum will by degrees be separated, and afterwards the sulphate of alkali, which was an ingredient into the alum, whether it has been pot-ash, or ammoniac. The sulphate of lime will fall first, because most difficultly soluble; the sulphate of pot-ash will be the next in order; and, lastly, the ammoniac, being the most soluble of all. If at the same time the alum under examination has contained a portion of sulphate of soda\*, it will be more difficult to separate this from the sulphate of ammoniac, because it is also very soluble. But it will presently be shewn, that no soda is contained in alum.

3. Pour into a saturated solution of alum, in six times its quantity of water (heated to about 120° Farenh.) a saturated solution of acetate of lead. Sulphate of lead will be produced, which falls down; and also, the acetate of alumine as well as the acetate of pot-ash (or of ammoniac), which, being readily soluble, remain dissolved in the water. The supernatant fluid must then be decanted from the sulphate of lead, and subjected to evaporation, until it becomes turbid; for by this means a little of the metallic sulphate, that was still held in solution by the water, is thrown down. When the fluid has been a second time filtered, it must be evaporated to dryness. The dry residuum consists of acetated argil and acetated pot-ash. And if it be then ignited, the acetic acid, together with the ammoniac, if present, will be volatilized. Water must now be poured upon the residue; which, if only ammoniac has entered into that compound, extracts nothing from it, by reason of the volatilization of this alkali, and of the insolubility of aluminous earth in water. But if a portion of vegetable alkali was contained in it, it is on account of its fixity, left behind in the argil, and will be dissolved in the water. The alkali receives some carbonic acid from the destruction of the acetic, but is still far from being saturated with it; and hence a part of the argil will be dissolved along with it by the water. The earth must, in that case, be precipitated, by saturating the solution with carbonic acid, or letting it stand for a long time in open vessels, slightly covered.

Analytical experiments alone are, nevertheless, insufficient to decide with certainty, whether all the three alkalis, or only one, or two, of them are by their accession capable of producing alum; and therefore our arguments cannot yet rest on a complete induction. For, supposing I had decomposed artificial as well as native alum in a thousand different ways, and had found no soda in it, I cannot fairly conclude that the mineral alkali is

\* I need not here observe, that salts of more difficult solubility are, for the most part, (though not perfectly) separated by the gradual and successive diminution of the water, and crystallization.

unfit for the production of alum. Not to mention the few species of alum, which I have examined, I should not choose to draw such an inference from the results I did obtain from them.

*Synthetical* experiments, on the contrary, I am persuaded, ought to decide in this case. And by these, (although *Marggraf* mentions, in a general way only, the pot-ash as a necessary requisite to the generation of sulphate of alumine, without excluding the soda) I have until the present moment been convinced, that the combination of soda with sulphuric acid and argil does *not* yield *any* alum, I mean that very salt, which is known by that name; but that it is generated by the addition of pot-ash.

I employed, in my experiments, native soda (natrum) from Hungary, purified in a high degree. It was this purity that induced me to institute those experiments. And to assure myself that no portion of pot-ash did interfere, I have precipitated the argil from Roman alum, not by means of pot-ash, but by soda. If, therefore, somewhat of the neutral sulphate, generated by the precipitation, had remained in the earth after it wasedulcorated, it must necessarily have been sulphate of soda.

The precipitated alumine was then properly washed; and I thought it needless to free it, according to *Richter*, from the adhering sulphuric acid, the presence of which was not detrimental in this instance. I then dissolved it in purified sulphuric acid, in excess, so that the solution reddened litmus-paper dipped in it. As I have already observed, the due proportion of the earth to the acid is found of itself, by permitting the acid, moderately diluted, to dissolve as much of the earth as it can in a low temperature. When I afterwards added to the filtered solution successive portions of dissolved carbonate of soda, till part of the alumine itself began to precipitate, I filtered the solution once more, and began to attempt the crystallization of the salt, by gradually repeated evaporation and alternate cooling.

But in none of these instances did I obtain alum; but rather a *very soluble salt*, altogether *different* in its *form*. This combination is of so easy solution, that I might at first have repeatedly suspected an accidental transposition of my evaporating dishes, if I had not been sure of the contrary, by the small quantity of the residual liquor, and its refusing to crystallize. That great solubility agrees very well with that of Glauber's salt (sulphate of soda) as *vice versa*, the difficult solution of alum corresponds with that sulphate of pot-ash. When, in one of my experiments, I added just so much of soda, that the lixivium began to acquire a permanent cloudiness (owing to the argillaceous particles first precipitated, and which are redissolved by the redundant acid, if the fluid be agitated, and thus the earthy particles be brought into contact \* with its not yet alkalized part), there resulted from its farther evaporation and cooling some minute, single, short needled crystals, resembling those of gypsum. These, however, merely contained aluminous, and no calcareous earth; for they readily dissolved in water; the oxalic acid precipitated nothing from their solution; pure

\* The earthy particles newly precipitated are, in general, more soluble, than those of a longer standing and desiccated; because the former are subtilely divided, and moistened through the whole of their mass.



(caustic) ammoniac, on the contrary, threw down the argillaceous earth they contained. And when the lye was still farther evaporated, it coalesced upon refrigeration into a coherent saline mass, in which, however, the needle-shaped texture could be distinguished. In another experiment, in which I added a little more of (the same) soda in such proportion that the quantity of precipitated argil became somewhat more considerable, I obtained by evaporating the filtered lixivium to a small remainder, and as it cooled, longish, flat, tabular six-sided crystals; (that is, very low hexagonal prisms, with broad ends and very low lateral facets, the two opposite ones of which were longer, and the other four shorter.) After the mother-water, which in proportion to the closely grouped crystals was but small in quantity, had been decanted, and the crystals washed with a little cold water, I attempted to take them out of the vessel by means of a silver spoon; but they were too soft, and were immediately compressed. Cold water (at 60° Farenh.) easily and quickly dissolved them; I have not yet determined the quantity required for that purpose; but certainly no more than *three* parts are necessary for *one* of the salt. The solution tasted like that of alum; it gave to paper, stained with litmus, of a faint red; (the undiminished solution, before the shooting of the crystals, afforded a stronger red); whence it follows, that there was a smaller proportion of the acid in the crystallized salt than in the whole fluid.

My attempts to produce alum by the acid of soda, being thus frustrated, I doubted very much whether *ammoniac*, the properties of which is far less analogous to those of pot-ash, than the properties of soda, would serve to produce alum, by combination with sulphuric acid and argillaceous earth. However, experience teaches that in this as well as in various other cases, reasonings from analogy prove very often incorrect, when resorted to in our enquiries into nature. Precipitate the argil from an aqueous solution of Roman alum, by means of ammoniac, wash the precipitate well; dissolve it then in pure sulphuric acid, in the manner pointed out above; and then continue adding ammoniac (either carbonated or caustic) by degrees, until the solution begins to assume a permanent turbidness. Let the whole then be reduced by evaporation so much, that for *one* part of the dissolved salt (the quantity of which may be ascertained by the quantity of alum employed at the beginning of the process) there shall remain from six to eight parts of water. As this lye cools, a salt will crystallize, which, with respect to its octahedral form, (in the best way to be perceived around the threads hung into the liquid) to the solidity and hardness of its crystals, and to its difficult solubility, &c. perfectly agrees with the alum, which is formed by means of pot-ash\*.

\* On this subject see this Journal, I. 313.



## II.

*Experiments on Whinstone and Lava. By Sir JAMES HALL, Bart. F.R.S. & F.A.S. Edin\*.*

(Concluded from page 18.)

THE suppositions which these gentlemen have thus advanced, and have seriously maintained in various parts of their works, have arisen in both from the belief, that, in our fires, nothing but glass can be produced from a lava after complete fusion. This being taken for granted, it would certainly be very difficult to explain the phenomena of actual eruptions, by means of the known agents of nature. Recourse has therefore been had, by one of these gentlemen, to a hypothetical modification of these agents; and by the other to the influence of substances, which have left behind them no trace of their existence †, and which, had they been present, could not have produced the effects ascribed to them.

According to both suppositions, the heat of volcanos is conceived to be of very little intensity; but the few observations I had occasion to make; which are confirmed by innumerable facts related by travellers, convince me that it must far exceed what is requisite for the most perfect fusion of the lavas, and of all the substances contained in them ‡; and the experiments already described supersede the necessity of supposing any thing different from the common course of nature; for they afford, analogically, an easy solution of the difficulty, by showing that glass is not the only result of fusion, and that whin, a substance like lava, when cooled slowly after fusion, resumes its stony character. But, not content with analogy alone, I resolved to ascertain the truth of these conclusions in a direct manner, and performed the following experiments with specimens of six different lavas, four of which, to my certain knowledge, had made part of external volcanic currents. In the present state of geology, too much pains cannot be bestowed in ascertaining that the specimens collected are really lavas, since this circumstance has been frequently overlooked, as I shall endeavour to shew, when I speak of the differences between them and whinstone.

\* It ought to have been noticed in the last Number that this paper is inserted in the Edinburgh Transactions, though, by some culpable neglect, the copies of that work have not yet reached London. N.

† None of the lavas I have seen contained the smallest vestige of petroleum; nor did I meet with any sulphur but what was evidently produced by the condensation of vapours, rising through crevices, long after the eruptions had ceased.

‡ I conceive, therefore, that the formation of the insulated substances contained in lavas, as well as the other peculiarities of internal structure, possessed by lavas in common with granite and basaltes, must be ascribed in all of them to crystallization during slow cooling after fusion, as I had stated formerly in Spring 1790, (*Transf. Edin. vol. III.*) The year following, Dr. Beddoes presented to the Royal Society of London a paper, in which he also explains the character of granite and basaltes by crystallization, in consequence of slow cooling.

No. 1. *Lava of Catania.*

This is the celebrated lava, which, in 1669, laid waste great part of the town of Catania. The interior part of the current (accurately described by M. Dolomieu, *Isles Ponces*, p. 256 \*), from which the subject of our experiment was taken, consists of a light grey basis, interspersed with crystals of felspar and of schorl, (augit). It bears a general resemblance to the rock of the basaltic columns on Arthur's Seat, and exhibited the same phenomena in our experiments. After strong heat, the whole was reduced, by rapid cooling, to pure black glass; but when the heat applied was moderate, the felspars remained unchanged. Being maintained, after a second fusion, in a temperature of 28, both these glasses yielded stony and crystallized substances, somewhat less fusible than the original; and when exposed to a temperature of 22, they crystallized rapidly, like most of the whins, into the liver crystallite. This last property is common to all the lavas.

No. 2. *Lava of Sta Venere.*

This current has flowed in the neighbourhood of a little chapel, called *S<sup>ta</sup> Venere*, above the village of Piedimonte, on the north side of Mount *Ætna*. Owing to the strong resemblance which it bears to stones supposed not volcanic, we took care that our specimens should be broken from the actual current; and to one of them, though mostly compact, is attached a scorified mass, which had made part of the external surface. The solid part is of a black, or rather dark blue, colour; very fine grained and homogeneous, having a multitude of minute and shining facettes visible in the sun; in this, and in other circumstances, it greatly resembles the rock of Edinburgh castle. This lava is the second in M. Dolomieu's *Catalogue*, and is well described, p. 186 †.

The pure black glass formed from this lava yielded, in the regulated heat, the most highly crystallized mass we obtained from any lava or whin.

No. 3. *Lava of La Motta di Catania.*

This is likewise compact and homogeneous, but for a number of small yellow grains of chrysolite scattered through it, (described by M. Dolomieu, p. 191 ‡). It has been thrown up by a partial eruption bursting through the sandstone hills which surround Mount *Ætna*. The situation of this mass is singular: it rests upon a little hill, formed of loose

\* “ Elle est formée d'une pâte de roche de corne grise, à grains fins, mêlée d'écailles, et de cristaux de feld-spath de même couleur; elle contient un très grand nombre de cristaux de schorl noir, et de grains de chrysolites jaunes, les uns et les autres quelquefois chatoyans, de différentes couleurs dans leurs fractures. . . . . Cette lave a une cassure sèche, et un grain rude, surtout dans le centre des courans; c'est là où elle a toujours conservé une couleur plus claire, qui doit être celle de sa base; sur les bords et les surfaces elle s'est fort noircie; elle y a acquis une assez forte action sur l'aiguille aimantée que celle du centre n'a presque point.”

† “ Lave homogène noire: son grain est fin et serré, il est un peu brillant, comme micacé lorsqu'on le présente au soleil; sa cassure nette et sèche est conchéide comme celle du silex.”

‡ It belongs to the fifth variety of his compact lavas.



scoria, the summit and sides of which are covered by the stony mass, so that no crater is visible. It struck me on seeing it, and I found M. Dolomieu had formed the same opinion, that the lava had risen up in a perpendicular direction, and had flowed over on all sides. Its great thickness, and small extent, seem to favour a conjecture which this naturalist has formed with regard to several lavas, that they were erupted at the bottom of an ocean which once covered Sicily, and, being quickly cooled by the contact of water, had been prevented from flowing far. The conjecture seems plausible enough\*; and, having no proof that this substance made part of an external current, as I have with respect to the first two mentioned, I do not exhibit it as a lava with the same confidence. Whatever be its history, however, it possesses the chemical properties common to whin and lavas.

Its glass yielded a dark grey crystallite of uniform texture. Beside it in the drawer, now on the table, I have placed a crystallite, formed from the whin No. 1. which resembles it in every respect.

#### No. 4. *Lava of Iceland.*

I received the specimen from a person who found it on the spot; but not being acquainted with the circumstances of its original position, I cannot be certain that it is a lava. It has however every appearance of being such.

It is a blue homogeneous substance, having some chrysolites scattered irregularly through it. Nearly half its bulk is occupied by large air holes, which do not appear to have contained any extraneous matter.

It produced a very fusible glass, from which was formed a crystallite much more refractory than the original.

#### No. 5. *Lava of Torre del Greco.*

This lava, which flowed from Vesuvius to the sea in the middle ages, has been an object of much attention, on account of its conspicuous basaltic form. It consists of a grey basis, the fracture of which is coarse and rough, and in which are embedded large and well characterized crystals of schorl, (augit), with a few chrysolites, (olivins).

It was found to be less fusible than any of the others, yet its glass crystallized in a lower temperature.

#### No. 6. *Lava of Vesuvius, eruption 1785.*

From the circumstances in which the above five lavas have been seen to crystallize after fusion, it can scarcely be doubted that the same process takes place in a volcanic stream, which in consequence of its bulk, must cool with considerable slowness, and that a vitreous character would be assumed by the whole mass, were it cooled with sufficient rapidity.

\* M. Dolomieu ascribes the formation of part of Mount Ætna itself to a similar cause. I shall have occasion, in another part of this paper, to consider that opinion.



The truth of this last opinion is demonstrated by some facts which I accidentally observed, long before my present views had occurred, when, in spring 1785, I had an opportunity of examining a stream of lava, which flowed from Vesuvius. The eruption was comparatively so gentle, that I was able, though not without inconvenience, to approach and examine the fiery stream on three different days. It was in general concealed by a thin white smoke, which the wind blew aside occasionally, so that I could distinctly see the lava as it burst from the side of the hill. It was then of a bright white heat, and flowed with the agility and rapidity of water, in all respects resembling melted iron running from the furnace. The liquid, at its first emergence, manifested a strong effervescence, which subsiding as the heat abated, shewed itself at last only in the bursting of some very large bubbles, accompanied with a white smoke. Where I approached the stream, it was still of a strong red heat, and had the consistence of honey. I thrust a stick into it with ease, to the end of which some of the lava adhering, by its viscosity, allowed itself to be drawn out into threads, and was found, when cold, to have a shining surface, and a vitreous fracture.

Being thus convinced that I had met with a lava of glass, I prepared some moulds of stucco, in which I meant to take casts with that rare substance; and with this view returned to the mountain. I found the stream was not so liquid as at first, but I was able, by means of a ladle fixed on the end of a pole, to lift the specimen now before us in a state like dough. I then pressed it with a seal, by which means, though too coarse to receive an accurate impression, it took the shape it now bears; which is that of the ladle. It is very porous, one-third of it nearly being occupied by air holes. It contains a great number of small white crystals of Vesuvian garnet, embedded in a black substance, which completely resembles the glass obtained in our experiments from lava by rapid cooling after fusion. Besides all their other properties, it possesses the fusibility of the glasses; since it softens completely at 18, that is, 14 or 15 degrees below the softening point of any of the stony lavas. Being exposed to the process of regulated cooling, it gave the same result as all the other lava glasses. In the lower points it yielded a liver crystallite infusible under 30, and in the higher a stony substance like a common lava or whin, and fusible only at 35.

What has been said is applicable to the interior parts of lavas; but I was at a loss to understand the state of their external surface, which, cooling much more rapidly, might be expected to possess a vitreous character; yet glass is not found on the surface of lavas, except in a very few cases, and has occurred only in a single spot on *Ætna*. This difficulty was removed, however, by the following consideration: though the surface of a lava cools with far more rapidity than the rest of the mass, yet, owing to the contact of the fiery stream, that rapidity can never be very great; and we must suppose that the temperature of the surface employs more than a minute or two in descending from 23 to 21. Where this happens, we have shown that the substance consolidates into the liver crystallite, which completely resembles the scoria of a lava. A small fragment of the mass, which I

took from the running stream, being placed in the temperature of 22, lost its vitreous character in two minutes, as already stated; and had the mass itself been allowed to remain but a very little longer in the stream, it would certainly have acquired, as well as the rest of the surface, the dull character of scoria.

The same property accounts for the crust which is formed on the surface of flowing lavas, and which constitutes so remarkable a feature in their history. Were lava to congeal after the manner of pitch or wax, by an uniform and gradual increase of viscosity throughout, no crust would be formed, or if, by the action of cold air, the upper surface were to harden a little, it might be softened again by an influx of fresh matter a very little hotter than itself. In lavas, however, as we have proved, when the surface cools down to 21, it rapidly congeals to a hard substance, capable of resisting any heat under 30. The crust thus formed serves as a pipe, within which the flowing lava is confined. In several places on *Ætna* we meet with vast galleries, along which, and out of which, the lava has flowed, leaving the crust entire \*.

The irregular manner in which a lava flows, when not extremely heated, may likewise be referred to the same cause. On the lower part of the running stream a crust is formed, so strong as to retard its progress during a certain time, but the liquid behind, accumulating by degrees, at last acquires sufficient strength to force open the crust; the lava then flows out with rapidity, and continues its course till it is again retarded by the formation of a new crust.

These experiments seem to establish, in a direct manner, what I had deduced, analogically, from the properties of whinstone, namely, that the stony character of a lava is fully accounted for by slow cooling after the most perfect fusion; and, consequently, that no argument against the intensity of volcanic fire can be founded upon that character. We are therefore justified in believing, as numberless facts indicate, that volcanic heat has often been of excessive intensity.

In the comparison instituted between whin and lava, the two classes are found to agree so exactly in all their properties which we have examined, as to lead to a belief of their absolute identity. This identity has been fully established by Dr. Kennedy, who has performed an exact analysis † of several of the very specimens of whinstone and lava mentioned in this paper; by which he discovered, that the elements of the two classes are the same: above all, that they both contain 4 or 5 *per cent.* of soda. Their agreement in this essential circumstance seems to account for their common properties, whilst the varieties of proportion, among their component elements correspond to the slight differences of result we have observed between the individuals of the same class ‡.

So

\* As at Malpertui above Piedimonte.

† An account of Dr. Kennedy's analysis is published in this volume. (Edin. Transf.)

‡ Though chemists have hitherto overlooked, in their experiments, the mode in which bodies were cooled after being reduced to a state of fusion; yet many results, which we are now entitled to ascribe to slow cooling, have been occasionally observed. The slag of a furnace bears a strong resemblance to what we have



So close a resemblance affords a very strong presumption in favour of Dr. Hutton's system, according to which both classes are supposed to have flowed by the action of heat; but the circumstances under which they were exposed to this action being materially different, we have reason to look for indications of that difference. Such are not wanting.

Calcareous spar frequently occurs in whinstone, either in veins or in detached nodules, but is never found in lava, and could not exist in a volcanic stream; for heat, in such circumstances, would infallibly drive off the carbonic acid, and compel the lime to unite with the other component elements of the mass. In whinstone, which Dr. Hutton supposes to have flowed, at some remote period, in crevices of the earth, at a great depth below what was then its surface, the weight and strength of the superincumbent mass of strata\* has been sufficient to resist the expansion of the carbonic acid, and to constrain it, upon the principle of Papin's digester, to continue in combination with the lime. This compound

have called the liver crystallite, and is probably formed in the same manner. I have seen a mass possessing, in a great measure, the stony character of whins and lavas, which was produced in a lime-kiln by the fusion of an impure limestone; and Dr. Beddoes has observed a crystallized texture in the slags of some iron furnaces. I am informed, that the celebrated Mr. Klaproth has described some striking examples of crystallization after fusion, which he obtained in exposing various substances to the heat of the porcelain furnace at Berlin.

\* It may be asked, what has become of this superincumbent mass; and by what means it has been removed. Dr. Hutton answers, that it has been gradually worn away during an immense course of ages, by the action of those causes which continue, under our eyes, to corrode the surface of the globe: that the solid parts, being conveyed to the bottom of the ocean, are there deposited in beds of sand and gravel, which, in some future revolution, being exposed to heat, may be again converted into stony strata.

The whole of this system appears to me well founded, except in what regards the removal of the superincumbent mass, which has been performed, I conceive, in a very different manner. I am inclined to agree on this point with M. Pallas, M. de Saussure, and M. Dolomieu, and to believe that, at some period very remote with respect to our histories, though subsequent to the induration of the mineral kingdom, the surface of the globe has been swept by vast torrents, flowing with great rapidity, and so deep as to overtop the mountains; that these torrents, by removing and undermining the strata in some places, and by forming in others immense deposits, have produced the broken and motley structure, which the loose and external part of our globe every where exhibits.

In the Alps and in Sicily I have witnessed several of those curious facts, upon which M. de Saussure and M. Dolomieu found their opinion, and which seem to justify their conclusions. I have likewise observed, in this country, many phenomena which denote the influence of similar agents. Lord Daer, who joins me in agreeing with Dr. Hutton in almost every article but this, has added great weight to the argument by some general observations on lakes, and by some very interesting facts which he has observed in the Highlands of Scotland. We propose to pursue this subject, and to lay the result of our inquiries before the society. Dr. Hutton, in the second volume of his *Theory of the Earth*, has taken great pains to refute all that has been said about these torrents; but, in my opinion, their existence is not only quite consistent with his general views, but seems deducible from his suppositions, almost as a necessary consequence. When the strata, according to his system, were elevated from the bottom of the sea, the removal of so much water, if not performed with unaccountable slowness, must have produced torrents, in all directions, of excessive magnitude, and fully adequate to the effects I have thus ascribed to them.

seems.



seems to have entered readily into fusion, along with the whinstone, but to have kept separate from it, as oil separates from water through which it has been diffused, thus giving rise to the spherical form, which the nodules of calcareous spar generally exhibit with more or less regularity\*.

This circumstance accounts for an appearance which has misled some of the early observers of our minerals. Many whinstone rocks externally resemble very porous lavas, but when broken are always found to be quite compact internally, and to contain numerous round nodules of calcareous spar. Near the surface, the nodules, being washed out by rain, have left the cavities which have given rise to this deception. The spherical form of the air holes in lavas, and of the nodules of calcareous spar in whins, seems to have been produced by a cause common to both, the mutual repulsion of two fluids intermixed, but not disposed to unite.

It must be owned, that this theory of calcareous spar is as yet hypothetical; but it is supported by strong analogy, and promises to be of service, by leading to decisive experiments and observations. I cannot help believing, that, by a careful examination of the volcanic countries, facts may yet be discovered which will throw light on this subject. In order to promote and direct such researches, I shall beg leave to state some observations which I made in those countries in 1785, before I was attached to any system of geology.

It is generally supposed, that some lavas of *Ætna* contain calcareous spar and zeolite; but this I conceive to be a mistake. It is true, as I have seen, that many rocks of *Ætna* contain these substances in abundance; but in my opinion these rocks are no lavas, but have flowed subterraneously like our whins, and are the same with them in every respect. A particular district of *Ætna*, comprehending the Cyclopiæ Isles, and the country round *La Trezza*, and the castle of *Jaci*, is decidedly of this description; and vestiges of the same kind occur in other parts of the mountain. In one place fossil coal has been found, and in another we saw marine shells. In the neighbourhood of *Bronte* we observed a high ridge formed of strata of sandstone and limestone, partly overflowed and concealed by recent lavas, but so placed as to render it evident that its continuation formed no inconsiderable part of the mountain. Thus, *Ætna* being composed, partly of the subterranean, and partly of the external productions of fire, may be expected to afford numberless opportunities of pursuing the comparison between these two classes†.

A most interesting scene for such a comparison occurs likewise on *Vesuvius*. The history of this volcano is simpler than that of *Ætna*, for it has been evidently formed, with all its appendages, by the continued action of external eruptions, which have raised it, at some remote period, from the bottom of a sea, occupying all the *Campi Phlegreæi*, and washing the surrounding *Appenines*. The whole volcano seems once to have consisted of

\* The modifications of the action of heat, occasioned by pressure, which have been taken into account by no geologist but *Dr. Hutton*, distinguish his theory from all other igneous theories.

† *M. Dolomieu* has observed this distinction; but supposes that the masses which we conceive to have flowed subterraneously were erupted at the bottom of the sea.

a single large cone, the greatest part of which has sunk during some violent eruption, probably that which took place in the time of Pliny, leaving a fragment of its basis, now called the mountain of Somma. This fragment retains its original shape; and on the side fronting the towns of Somma and Otaiano, the external conical surface, along which the ancient lavas had flowed, is still entire. Fronting the centre of the cone, Somma breaks off abruptly, and presents a vertical cragg, some hundred feet in height, which is concave inwards. From the gulf, produced by the ruin of the ancient mountain, though not exactly from its centre, have arisen the explosions which, by repeated accumulation, have formed the present cone of Vesuvius. Next the sea, this cone has extended itself so as completely to cover all remains of the ancient one, forming a continued slope from the crater to the foot of the mountain. On the opposite side it meets the base of the craggs of Somma, and forms an angle, into which many successive streams of lava have flowed, producing a narrow horizontal valley, in the form of a crescent, called the *Atrio del Cavallo*. From this valley the craggs of Somma present a complete view of the internal structure of the ancient mountain, corresponding, in most things, to what might have been supposed.

The various substances, deposited successively on the external surface of the ancient cone, being cut vertically in this cragg, their succession is distinctly seen, the section of each stratum presenting to the view part of a horizontal circle; the whole consists of alternate layers of thin streams of lava, and very thick beds of loose frothy *rapilli*, which last being thrown into the air in a soft state, had fallen in showers on the sides of the mountain.

In various places the regularity of this arrangement is interrupted by certain vertical lavas, from two feet to ten or twelve in thickness, which cross the strata just described in an irregular manner, and pass upward, without distinction, through the solid beds, and through the loose ones. It immediately occurred to us\*, that these lavas must have flowed in fissures of the ancient mountain; and we accounted for them by supposing, that a melted stream, flowing along the external surface, had met in its course with one of those crevices which are formed in all great eruptions, and had flowed into it so as to return again into the heart of the mountain. This conjecture very nearly agrees with those advanced by M. Dolomieu, and by M. Breislack, who both mention these vertical lavas of Somma†.

I have since been induced to consider this phenomenon, which formerly seemed to present only an amusing variety in the history of volcanic eruptions, as of the utmost consequence in geology, by supplying an intermediate link between the external and the subter-

\* I saw this place in company with Dr. J. Home in 1785.

† M. Dolomieu conceives these lavas to have flowed over the lips of the crater, (*Iles Poncez*, p. 100.); M. Breislack, that they had first filled the open cavity of the crater, and from thence had flowed into crevices formed in its sides, "che una lava avendo riempita la cavita del cratere si fosse insinuata per queste fenditure," (*Topografia Fisica della Campania*, p. 115.) This last mentioned work, published in 1798, contains many interesting and accurate descriptions. Should the circumstances of the times permit, the author will have it in his power to follow out, with every advantage, the hints I have suggested.



aneous productions of heat. I now think, that, though we judged rightly in believing those lavas to have flowed in crevices, we were mistaken as to their direction; for instead of flowing downwards, I am convinced they have flowed upwards, and that the crevices have performed the office of pipes, through which lateral explosions have found a vent. This will appear in the highest degree probable, when we attend to the known history of volcanic eruptions. It generally happens, that the lava begins to flow from the summit, in consequence of the crater being filled with liquid matter up to the brim. At that moment the basis of the mountain must be pressed outwards by a very great hydrostatical force, equal to the weight of a column of liquid lava as high as the mountain itself. It is natural then to expect, that this pressure, assisted by strong percussions of explosion, should lacerate the body of the hill, and form great rents. The lava, urged upwards by the same pressure, would flow through these rents, and emerge at the surface with violence. The discharge would continue through this channel till the propelling force had ceased, when the rents would be left full of lava; which, cooling in that position, would produce vertical lavas, such as those of Somma. This supposition is confirmed by various phenomena: the lava ceases to flow from the crater as soon as a lateral eruption has begun; when it rushes with such violence from the side of the mountain as to fly to a great height into the air, like a *jet d'eau*; and it often makes its appearance, in the same instant, at various mouths, which are not scattered at random, but placed in one continued line, indicating the discharge from a rent. Some circumstances likewise, which I observed on a close examination of the vertical lavas, indicate that the crevices had performed the office of pipes. Frequently the substance at the middle differs from that on each side, whilst the sides resemble each other exactly. I explain this, by supposing that the lava, which had first flowed through the pipe, and had coated its sides with solid matter, had been followed by a stream somewhat different, which had remained there on the cooling of the whole. In one case, I found lava on each side, and in the middle tuffa, which is generally supposed to have been erupted in the state of watery mud. In another, the substance in immediate contact with the mountain is vitreous, the rest being common lava. This is fully explained by our experiments, if we suppose the stream to have flowed into a cold crevice.

To apply these observations to the general history of the globe: it is evident that the vertical lavas bear the closest resemblance, in point of position, to veins of every description, which, in all parts of the world, are found penetrating the strata, and which, according to Dr. Hutton, have flowed by means of subterraneous heat. The veins, or dikes, (as they are called), of whinstone, which so commonly occur in this country, differ from them in no circumstance which I had the means of observing. It is therefore natural to expect that, if examined with particular care, their agreement will be found complete. Of this, however, we must not form too sanguine expectations; for though the vertical lavas of Somma have undoubtedly sustained the pressure of a great superincumbent mass, we have no proof that this force was sufficiently strong, as Dr. Hutton supposes was the case in whinstone, to repress the volatility of carbonic acid. On the other hand, as we are yet



yet entirely ignorant of the degree of force requisite for this purpose, we have no proof that it has been too weak. All the veins of Somma I examined were absolutely compact, except one which was full of pores. I am unable to determine whether this was the real porosity of a lava, or whether, as in our whins, it arose from the removal, near the surface, of nodules of calcareous spar. Granting that these pores were real air holes, the circumstance was peculiar to that single stream, and may have been owing to an inferior degree of pressure; for, in this respect, we have reason to look for the greatest diversity. Some vertical streams must have flowed while the mountain was yet low; others may have found vent at a low level; in both which cases, the pressure would be feeble: whereas other streams, communicating with elevated lateral eruptions, would sustain, in their lower parts, the full re-action of deep columns of liquid lava, and may be expected to exhibit the effects of great pressure. Should any future traveller be fortunate enough to meet with a nodule of calcareous spar in a lava, occupying the crevice of a mountain formed by undoubted external eruptions, all that has been said of the effects of pressure would cease to be hypothetical, and this fundamental article of Dr. Hutton's theory would be established beyond dispute.

I have now examined the relation between whinstone and lava in various points of view; and the result of the investigation, by showing the intimate connection between the two classes, tends strongly to confirm the ideas of Dr. Hutton. I flatter myself, likewise, that the experiments, independently of the general views of geology, are of some value, by accounting for the stony character of lavas, and thus enabling us to dispense with the various mystical suppositions which have of late perplexed the history of volcanic phenomena.

## TABLE OF FUSIBILITIES.

	<i>Substances.</i>	<i>Original softened.</i>	<i>Glass softened.</i>	<i>Crystalline softened.</i>
No. 1.	Whin of Bell's Mills Quarry - - - - -	40	15	32
2.	Whin of Castle Rock - - - - -	45	22	35
3.	Whin of Basaltic Column, Arthur's Seat - - - - -	55	18	35
4.	Whin near Duddingstone Loch - - - - -	43	24	38
5.	Whin of Salisbury Craig - - - - -	55	24	38
6.	Whin from the Water of Leith - - - - -	55	16	37
7.	Whin of Staffa - - - - -	38	14½	35
No. 1.	Lava of Catania - - - - -	33	18	38
2.	Lava of S <sup>a</sup> Venere, Piedimonte - - - - -	32	18	36
3.	Lava of La Motta - - - - -	36	18	36
4.	Lava of Iceland - - - - -	35	15	43
5.	Lava of Torre del Greco - - - - -	40	18	28
6.	Lava of Vefuvius, 1785 - - - - -	18	18	35

## III.

*A Memoir on the Discovery of certain Bones of a Quadruped of the Clawed Kind in the Western Parts of Virginia. By THOMAS JEFFERSON, Esq.*

(Concluded from page 43.)

THESE bones only enable us to class the animal with the unquiculated quadrupeds; and of these the lion being nearest to him in size, we will compare him with that animal, of whose anatomy Monsieur Daubenton has furnished very accurate measures in his tables at the end of Buffon's Natural History of the Lion. These measures were taken, as he\* informs us, from "a large lion of Africa," in which quarter the largest† are said to be produced. I shall select from his measures only those where we have the corresponding bones, converting them into our own inch and its fractions, that the comparison may be more obvious: and to avoid the embarrassment of designating our animal always by circumlocution and description, I will venture to refer to him by the name of the Great-Claw or Megalonyx, to which he seems sufficiently entitled by the distinguished size of that member.

	Megalonyx Inches.	Lion. Inches.
Length of the ulna, or fore-arm	20.1	13.7
Height of the olecranon	3.5	1.85
Breadth of the ulna, from the point of the coronoide apophysis to } the extremity of the olecranon	9.55	
Breadth of the ulna at its middle	3.8	
Thickness at the same place	1.14	
Circumference at the same place	6.7	
Length of the radius	17.75	12.37
Breadth of the radius at its head	2.65	1.38
Circumference at its middle	7.4	3.62
Breadth at its lower extremity	4.05	1.18
Diameter of the lower extremity of the femur at the base of the } two condyles	4.2	2.65
Transverse diameter of the larger condyle at its base	3	
Circumference of both condyles at their base	11.65	
Diameter of the middle of the femur	4.25	1.15
Hollow of the femur at the same place	1.25	
Thickness of the bone surrounding the hollow	1.5	
Length of the longest claw	7.5	1.41
Length of the second phalanx of the same	3.2	1.41

\* Buffon, XVIII. 38. Paris edition in 31 vols. 12mo.

† 2. De Manet, 117.



The dimensions of the largest of the foot bones are as follow:

	Inches.
Its greatest diameter, or breadth at the joint	2.45
Its smallest diameter, or thickness at the same place	2.28
Its circumference at the same place	7.1
Its circumference at the middle	5.3

	Of longest toe.	Middle sized toe.	Shortest toe.
2d. Phalanx. Its length	3.2	2.95	
Greatest diameter at its head or upper joint	1.84	2.05	
Smallest diameter at the same place	1.4	1.54	
Circumference at the same place	5.25	5.8	
3d. Phalanx. Its length	*7.5	†5.9	3.5
Greatest diameter at its head or upper joint	2.7	2.	1.45
Smallest diameter at the same place	.95	.9	.55
Circumference at the same place	6.45	4.8	

Were we to estimate the size of our animal by a comparison with that of the lion on the principle of *ex pede Herculem*, by taking the longest claw of each as the module of their measure, it would give us as a being out of the limits of nature. It is fortunate therefore that we have some of the larger bones of the limbs which may furnish a more certain estimate of his stature. Let us suppose then that his dimensions of height, length and thickness, and of the principal members composing these, were of the same proportions with those of the lion. In the table of M. Daubenton an ulna of 13.78 inches belonged to a lion 42½ inches high over the shoulders: then an ulna of 20.1 inches bespeaks a megalonyx of 5 feet 1.75 inches height, and as animals who have the same proportions of height, length, and thickness have their bulk or weights proportioned to the cubes † of any one of their dimensions, the cube of 42.5 inches is to 262 lb. the height and weight of M. Daubenton's lion as the cube of 61.75 inches to 803 lb. the height and weight of the megalonyx; which would prove him a little more than three times the size of the lion, I suppose that we should be safe in considering, on the authority of M. Daubenton, his lion as a large one. But let it pass as one only of the ordinary size, and that the megalonyx, whose bones happen to have been found, was also of the ordinary size. It does § appear that there was dissected for the academy of sciences at Paris, a lion of 4 feet 9½ inches height. This individual would weigh 644 lb. and would be in his species, what a man of eight feet height would be in ours. Such men have existed. A megalonyx equally monstrous would be 7 feet high, and would weigh 2000 lb. but the ordinary race, and not the monsters of it, are the object of our present enquiry.

\* It is actually 6½ inches long, but about ¼ inch appear to have been broken off.

† Actually 5.65 but about ¼ inch is broken off.

‡ Buffon xxii. 121.

§ Buffon xviii. 15.



I have used the height alone of this animal to deduce his bulk, on the supposition that he might have been formed in the proportions of the lion. But these were not his proportions, he was much thicker than the lion in proportion to his height, in his limbs certainly, and probably therefore in his body. The diameter of his radius, at its upper end, is near twice as great as that of the lion, and, at its lower end, more than thrice as great, which gives a mean proportion of  $2\frac{1}{2}$  for 1. The femur of the lion was less than  $1\frac{1}{4}$  inch diameter. That of the megalonyx is  $4\frac{1}{4}$  inches, which is more than three for one. And as bodies of the same length and substance have their weights proportioned to the squares of their diameters, this excess of caliber compounded with the height, would greatly aggravate the bulk of this animal. But when our subject has already carried us beyond the limits of nature hitherto known, it is safest to stop at the most moderate conclusions, and not to follow appearances through all the conjectures they would furnish, but leave these to be corroborated or corrected by future discoveries. Let us only say then, what we may safely say, that he was *more* than three times as large as the lion: that he stood as pre-eminently at the head of the column of clawed animals as the mammoth stood at that of the elephant, rhinoceros, and hippopotamus: and that he may have been as formidable an antagonist to the mammoth as the lion to the elephant.

A difficult question now presents itself. What is become of the great-claw? Some light may be thrown on this by asking another question. Do the wild animals of the first magnitude in any instance fix their dwellings in a thickly inhabited country? such, I mean, as the elephant, the rhinoceros, the lion, the tyger? as far as my reading and recollection serve me, I think they do not: but I hazard the opinion doubtingly, because it is not the result of full enquiry. Africa is chiefly inhabited along the margin of its seas and rivers. The interior desert is the domain of the elephant, the rhinoceros, the lion, the tyger. Such individuals as have their haunts nearest the inhabited frontier, enter it occasionally, and commit depredations when pressed by hunger: but the mass of their nation (if I may use the term) never approach the habitation of man, nor are within reach of it. When our ancestors arrived here, the Indian population, below the falls of the rivers, was about the twentieth part of what it now is. In this state of things, an animal resembling the lion seems to have been known even in the lower country. Most of the accounts given by the earlier adventurers to this part of America make a lion one of the animals of our forests. Sir John Hawkins \* mentions this in 1564. Thomas Harriot, a man of learning, and of distinguished candor, who resided in Virginia in 1587 † does the same, so also does Bullock in his account of Virginia, ‡ written about 1627, he says he drew his information from Pierce, Willoughby, Claiborne, and others who had been here, and from his own father who had lived here twelve years. It does not appear whether the fact is stated on their own view, or on information from the Indians, probably the latter. The progress of

\* Hakluyt, 541. edition of 1589.

† Ibid. 757, and Smith's History of Virginia, 10.

‡ Bullock, page 5.

the new population would soon drive off the larger animals, and the largest first. In the present interior of our continent there is surely space and range enough for elephants and lions, if in that climate they could subsist; and for mammoths and megalonyxes who may subsist there. Our entire ignorance of the immense country to the west and north-west, and of its contents, does not authorise us to say what it does not contain.

Moreover it is a fact well known, and always susceptible of verification, that on a rock on the bank of the Kanhawa, near its confluence with the Ohio, there are carvings of many animals of that country, and among these one which has always been considered as a perfect figure of a lion. And these are so rudely done as to leave no room to suspect a foreign hand. This could not have been of the smaller and maneless lion of Mexico and Peru, known also in Africa both in \* ancient and † modern times, though denied by ‡ M. de Buffon: because like the greater African lion, he is a tropical animal; and his want of a mane would not satisfy the figure. This figure then must have been taken from some other prototype, and that prototype must have resembled the lion sufficiently to satisfy the figure, and was probably the animal the description of which by the Indians made Hawkins, Harriot, and others conclude there were lions here. May we not presume that prototype to have been the great-claw?

Many traditions are in possession of our upper inhabitants, which themselves have heretofore considered as fables, but which have regained credit since the discovery of these bones. There has always been a story current that the first company of adventurers who went to seek an establishment in the county of Greenbriar, the night of their arrival were alarmed at their camp by the terrible roarings of some animal unknown to them: that he went round and round their camp, that at times they saw his eyes like two balls of fire, that their horses were so agonised with fear that they couched down on the earth, and their dogs crept in among them, not daring to bark. Their fires, it was thought, protected them, and the next morning they abandoned the country. This was little more than 30 years ago.—In the year 1765, George Wilson and John Davies, having gone to hunt on Cheat river, a branch of the Monongahela, heard one night, at a distance from their camp, a tremendous roaring, which became louder and louder as it approached, till they thought it resembled thunder, and even made the earth tremble under them. The animal prowled round their camp a considerable time, during which their dogs, though on all other occasions fierce, crept to their feet, could not be excited from their camp, nor even encouraged to bark. About day-light they heard the same sound repeated from the knob of a mountain about a mile off, and within a minute it was answered by a similar voice from a neighbouring knob. Colonel John Stewart had this account from Wilson in the year 1769, who was afterwards Lieutenant Colonel of a Pennsylvania regiment in the revolution-war; and some years after from Davies, who is now living in Kentucky.

\* Aristot. Animal, 9. 4. Pliny, 8. 16.

† Kolbe.

‡ Buffon, xviii. 18.



These circumstances multiply the points of resemblance between this animal and the lion. M. de la Harpe of the French Academy, in his abridgment of the General History of Voyages, speaking of the Moors, says\*, "it is remarkable that when, during their huntings, they meet with lions, their horses, though famous for swiftness, are seized with such terror that they become motionless, and their dogs equally frightened, creep to the feet of their master, or of his horse." Mr. Sparrman in his voyage to the Cape of Good Hope, chap. 11. says, "we could plainly discover by our animals when the lions, whether they roared or not, were observing us at a small distance. For in that case the hounds did not venture to bark, but crept quite close to the Hottentots; and our oxen and horses sighed deeply, frequently hanging back, and pulling slowly with all their might at the strong straps with which they were tied to the waggon. They also laid themselves down on the ground, and stood up alternately, as if they did not know what to do with themselves, and even as if they were in the agonies of death." He adds that "when the lion roars, he puts his mouth to the ground, so that the sound is equally diffused to every quarter." M. de Buffon (xviii. 31.) describes the roaring of the lion as, by its echoes resembling thunder: and Sparrman c. 12. mentions that the eyes of the lion can be seen a considerable distance in the dark, and that the Hottentots watch for his eyes for their government. The phosphoric appearance of the eye in the dark seems common to all animals of the cat kind.

The terror excited by these animals is not confined to brutes alone. A person of the name of Draper had gone in the year 1770, to hunt on the Kanhawa. He had turned his horse loose with a bell on, and had not yet gone out of hearing when his attention was recalled by the rapid ringing of the bell. Suspecting that Indians might be attempting to take off his horse, he immediately returned to him, but before he arrived he was half eaten up. His dog scenting the trace of a wild beast, he followed him on it, and soon came in sight of an animal of such enormous size, that though one of our most daring hunters and best marksmen, he withdrew instantly, and as silently as possible, checking and bringing off his dog. He could recollect no more of the animal than his terrific bulk, and that his general outlines were those of the cat kind. He was familiar with our animal misnamed the panther; with our wolves and wild beasts generally, and would not have mistaken nor shrunk from them.

In fine, the bones exist: therefore the animal has existed. The movements of nature are in a never ending circle. The animal species which has once been put into a train of motion, is still probably moving in that train. For if one link in nature's chain might be lost, another and another might be lost, till this whole system of things should vanish by piece-meal; a conclusion not warranted by the local disappearance of one or two species of animals, and opposed by the thousands and thousands of instances of the renovating power constantly exercised by nature for the reproduction of all her subjects, animal, ve-

\* Gentleman's, and London Magazines, for 1783.



getable, and mineral. If this animal then has once existed, it is probable on this general view of the movements on nature that he still exists, and rendered still more probable by the relations of honest men applicable to him and to him alone. It would indeed be but conformable to the ordinary economy of nature to conjecture that she had opposed sufficient barriers to the too great multiplication of so powerful a destroyer. If lions and tygers multiplied as rabbits do, or eagles as pigeons, all other animal nature would have been long ago destroyed, and themselves would have ultimately extinguished after eating out their pasture. It is probable then that the great-claw has at all times been the rarest of animals. Hence so little is known, and so little remains of him. His existence however being at length discovered, enquiry will be excited, and further information of him will probably be obtained.

The Cosmogony of M. de Buffon supposes that the earth, and all the other planets primary and secondary, have been masses of melted matter struck off from the sun by the incidence of a comet on it: that these have been cooling by degrees, first at the poles, and afterwards more and more towards their equators: consequently that on our earth there has been a time when the temperature of the poles suited the constitution of the elephant, the rhinoceros, and hippopotamus; and in proportion as the remoter zones became successively too cold, these animals have retired more and more towards the equatorial regions, till now that they are reduced to the torrid zone as the ultimate stage of their existence. To support this theory, he \* assumes the tusks of the mammoth to have been those of an elephant, some of his teeth to have belonged to the hippopotamus, and his largest grinders to an animal much greater than either, and to have been deposited on the Missouri, the Ohio, the Holston, when those latitudes were not yet too cold for the constitutions of these animals. Should the bones of our animal, which may hereafter be found, differ only in size from those of the lion, they may on this hypothesis be claimed for the lion, now also reduced to the torrid zone, and its vicinities, and may be considered as an additional proof of this system; and that there has been a time when our latitudes suited the lion as well as the other animals of that temperament. This is not the place to discuss theories of the earth, nor to question the gratuitous allotment to different animals of teeth not differing in any circumstance. But let us for a moment grant this with his former postulata, and ask how they will consist with another theory of his “qu’il y a dans la combinaison des elements et des autres causes physiques, quelque chose de contraire a l’aggrandissement de la nature vivante dans ce nouveau monde; qu’il y a des obstacles au developpement et peutetre a la formation des grands germes †.” He says that the mammoth was an elephant, yet ‡ two or three times as large as the elephants of Asia and Africa: that some of his teeth were those of a hippopotamus, yet of a hippopotamus § four times as large as those of Africa: that the mammoth himself, for he still considers him as a distinct animal ||, “was of a size

\* Buffon, Epoq. 2. 233, 234.

† Buffon, xviii. 145.

‡ 2. Epoq. 223.

§ 1. Epoq. 246. 2. Epoq. 232.

|| 2. Epoq. 234, 235.

superior to that of the largest elephants. That he was the primary and greatest of all terrestrial animals." If the bones of the megalonyx be ascribed to the lion, they must certainly have been of a lion of more than three times the volume of the African. I delivered to M. de Buffon the skeleton of our palmated elk, called original or moose, feet high over the shoulders, he is often considerably higher. I cannot find that the European elk is more than two-thirds of that height: consequently not one-third of the bulk of the American. He \* acknowledges the palmated deer (daim) of America to be larger and stronger than that of the Old World. He † considers the round horned deer of these States and of Louisiana as the roe, and admits they are of three times his size. Are we then from all this to draw a conclusion, the reverse of that of M. de Buffon. That nature, has formed the larger animals of America, like its lakes, its rivers, and mountains, on a greater and prouder scale than in the other hemisphere? Not at all, we are to conclude that she has formed some things large and some things small, on both sides of the earth, for reasons which she has not enabled us to penetrate; and that we ought not to shut our eyes upon one half of her facts, and build systems on the other half.

To return to our great-claw; I deposit his bones with the Philosophical Society, as well in evidence of their existence and of their dimensions, as for their safe-keeping; and I shall think it my duty to do the same by such others as I may be fortunate enough to obtain the recovery of hereafter.

TH: JEFFERSON.

*Monticello. Feb. 10th, 1797.*

P. S. *March 10th, 1797.* After the preceding communication was ready to be delivered in to the Society, in a ‡ periodical publication from London I met with an account and drawing of the skeleton of an animal dug up near the river La Plata in Paraguay, and now mounted in the cabinet of Natural History of Madrid. The figure is not so done as to be relied on, and the account is only an abstract from that of Cuvier and Roume. This skeleton is also of the clawed-kind, and having only four teeth on each side above and below, all grinders, is in this account classed in the family of unquiculated quadrupeds destitute of cutting teeth, and receives the new denomination of megatherium, having nothing of our animal but the leg and foot bones, we have few points for a comparison between them. They resemble in their stature, that being 12 feet 9 inches long, and 6 feet 4½ inches high, and ours by computation 5 feet 1.75 inches high: they are alike in the colossal thickness of the thigh and leg bones also. They resemble too in having claws: but those of the figure appear very small, and the verbal description does not satisfy us whether the claw-bone, or only its horny cover be large. They agree too in the circumstance of the two bones of the fore-arm being distinct and moveable on each other; which however is believed to be so usual as to form no mark of distinction. They differ in the

\* Buffon, xxix. 245.

† Ibid. xii. 91: 92. xxix. 245. Vide Suppl. 201.

‡ Monthly Magazine, Sep. 1796.

following



following circumstances, if our relations are to be trusted. The megatherium is not of the cat form, as are the lion, tyger, and panther, but is said to have striking relations in all parts of its body with the bradypus, dasypus, pangolin, &c. According to analogy then, it probably was not carnivorous, had not the phosphoric eye, nor leonine roar. But to solve satisfactorily the question of identity, the discovery of fore-teeth, or of a jaw bone shewing it had, or had not, such teeth, must be waited for, and hoped with patience. It may be better, in the mean time, to keep up the difference of name.

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IV.

*A Letter from Mr. JOHN HECKEWELDER, to Dr. BARTON, giving some account of the remarkable Instinct of a Bird called the Nine-Killer\*.*

Bethlehem, December 18th, 1795.

HAVING an opportunity by a friend of mine to Philadelphia, I must mention to you a curious fact, that came to my knowledge but yesterday.

I went to a farm, about eleven miles and a half from this place, to view a young orchard, which had been planted, about five weeks ago, under my direction, where on viewing the trees, I found, to my great astonishment, almost on every one of them, one and on some two and three grasshoppers, stuck down on the sharp thorny branches, which were not pruned when the trees were planted. I immediately called the tenant, and asked the reason and his opinion of this. He was much surprised at my ignorance about the matter, and informed me, "that these grasshoppers were stuck up by a small bird of prey, which the Germans called *Neun-toedter* (in English, Nine-killer); that this bird had a practice of catching and sticking up nine grasshoppers a day, and that as he well knew they did not devour the grasshoppers, nor any other insects, he thought they must do it for pleasure. I asked him for a description of this bird, and was perfectly satisfied that it lived entirely on small animals, such as small birds, mice, &c. for I had paid attention to this bird as early as the year 1761, when, in the winter, one of the same species took a favourite little bird out of my cage at the window, from which time I have watched them more closely, and have found them more numerous in the western-country than here. Not being satisfied with what the tenant had told me respecting the intention of the bird's doing all this (viz. for diversion sake), and particularly observing each and every one of these grasshoppers stuck up so regularly, and in their natural position as when on the ground, not one of them having its back downwards, I began to conjecture what might be the real intention which the bird had in this, and my determined opinion was, that this little bird-

\* Addressed to Dr. Benj. Smith Barton, and inserted in the Transactions of the American Philosophical Society, IV. 124:



hawk, by instinct, made use of this art, in order to decoy the smaller birds, which feed on insects, and by these means have a fair opportunity of catching them. All this I communicated to my friends, on my return home, and they were not less astonished at what I had related to them, than I had been on discovering the fact. It being agreed that one or more gentlemen of learning and observation should more minutely examine into this matter, the proprietor of this farm, with another gentleman and myself, went this day out for the purpose, and viewing the grasshoppers on a number of these small trees (some of which we cut off, and took home), we returned to the tenant, who not only himself but also his father and sister gave us the best assurances, that they had, long since, and from time to time, observed this bird catching grasshoppers and sticking them up in the manner already related, and that sometimes they had observed, in places where this species of bird keeps, numbers of grasshoppers stuck up on a thorn-bush in like manner. The Reverend Mr. V. Vleck is perfectly satisfied that this bird-hawk is the *Lanius Canadensis* (in Bartram \*), and has obligingly communicated the following account of this little bird-hawk to me: it is extracted from a German publication printed at Goettingen, in 1778, under the title of "Natural History for Children, by M. George Christian Paff," who after giving a description of the different species of this bird, concludes thus: "Why is this bird of prey called the nine-killer? Because it is said to have the habit of sticking beetles or other insects, and perhaps sometimes nine of them in succession, upon thorns, that they may not escape until he has leisure to devour them all at once. And for the same reason, it is sometimes called the thorn-sticker." Now by the above account, we see that it is known in Europe that this same species of birds actually does stick up insects of different kinds on thorns, &c. but it is supposed they eat them immediately after being stuck up. Here the case is quite otherwise. They remain stuck up, for we must suppose these to have been stuck up at least some weeks ago, and before the hard frosts set in. The very birds (as we suppose) that stuck them up are now on the same ground, watching the smaller birds that come out to feed, and have been seen catching the latter but a few days ago. If it were true, that this little hawk had stuck them up for himself; how long would he be feeding on one or two hundred grasshoppers? But if it be intended to seduce the smaller birds to feed on these insects, in order to have an opportunity of catching them, that number, or even one half, or less, may be a good bait all winter: and all of us, who have considered these circumstances, are firmly of opinion, that these insects thus stuck up, are to serve as a bait, &c. through the course of the winter.

You will readily excuse my being so lengthy on this subject. The matter appeared to me of too much consequence to pass over hastily. I shall be glad to hear your opinion on this subject.

\* I do not find that Mr. Bartram has mentioned, in any part of his *Travels*, a *Lanius Canadensis*. Since the date of this letter, Mr. Heckewelder has favoured me with a well-preserved specimen of the bird-hawk. It proves to be the *Lanius Excubitor* of Linnæus, the great-shrike of Mr. Pennant. B. S. B.

I send

I send you a few of these grasshoppers, as I cut them from the trees. They being hard and dry, most of their legs broke off in taking them home.

I am, with great respect,

Dear Sir,

Your most obedient and

Humble Servant,

JOHN HECKEWELDER.

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V.

*Letter from Dr. BEDDOES on the Experiments made at the Royal Institution with the Nitrous Oxide.*

To Mr. NICHOLSON.

SIR,

*Clifton, 17th April.*

**T**HOUGH I must be too well aware of the causes of variation in medical and physiological testimony, to be much moved by groundless contradiction, yet I own I did expect that competent experimenters would nearly agree in their reports of the effect, produced by respiring the *nitrous oxide*.

A few days ago, however, I was well informed, that on trial at a respectable institution in London, the gas had fallen far short of what Mr. Davy and myself had taught the public to expect. The gentleman who brought this intelligence being at Clifton, it was easy to give him personal proof of our fidelity.

Yesterday my friend Mr. William Reynolds, of Ketley, in Shropshire, assured me that he had himself respired the gas at the *institution* in London to no purpose; and that most others had done the same. The agency of the nitrous oxide is much too distinct and certain to leave a doubt, but that there must be some strange mistake here. On seeing a paralytic patient take it, Mr. W. R. was at once aware of the reason (at least, of one sufficient reason) for the failures in town, and on respiring a very small quantity indeed, he felt effects exactly the same in kind, as the agreeable ones described in my *notice*. He had previously inhaled atmospheric air without knowing it to be such. He expatiated with satisfaction on the sensations *now* excited by the nitrous oxide, and said he should like another dose. On being asked, if he had felt any such desire after the experiment in town, he answered, *no not the least*. We are constantly accustomed to the eager expression of this desire.



In our ample experience, we have not found the experiment to fail above once in twenty times. With a given degree of accuracy and perseverance, it would, I apprehend, *never* fail. I do not mean that the result would be always agreeable.

The gas being procured genuine, the lungs should be emptied by a strong expiration, the nose held tight, and the lips exactly closed on the mouth-piece. From 5 to 8 quarts breathed backwards and forwards into the reservoir (a proceeding of which Mr. Davy's researches will shew the propriety) have proved an average dose. The feelings will determine the time. To the hysterical and the exquisitely sensible, the gas should not be administered.

It is very well to know from actual trial, what the London experiments prove, that interrupted respirations, during which the gas is probably much diluted with residuary air in the lungs, can seldom have effect. This, however, an observer of any sagacity could have predicted.

A laborious analysis, and the result of a vast variety of careful observations, will ere long appear, to correct the misconceptions which may have arisen from loose casual trials. Meanwhile, I beg you to insert this explanation in your valuable Journal. I think it of no small importance to mankind, that there should exist nothing to prevent this great agent from being employed, whenever direct experience or circumstantial analogy warrants its use.

I am, Sir,

Respectfully your's,

THOMAS BEDDOES.

## VI.

*On the colouring Matter of Dog's Mercury (Mercurialis perennis Lin.)* By a Correspondent.

TO MR. NICHOLSON.

SIR,

**R**ESARCHES concerning the colouring principles of vegetable substances, have been very general and extensive; but I have never read any thing concerning the colour which resides in the juice of dog's mercury, and therefore conclude, that the properties of this vegetable are not very generally known.

The herb grows plentifully in woods and hedges. Its medical virtues are at present neglected; and it is never removed from its solitary abode for any useful purpose, but the instruction of the botanist. Accident may, perhaps, have disclosed this fact to him, that its juice affords a blue stain or colour: any one, however, may ascertain its truth by the following easy experiment:

Take



Take a stalk of the herb; rub the broken end upon a piece of fine white paper; and squeeze out some of the juice. No difference will be perceived at first, where the juice has touched the paper, but some hours afterwards it will be stained blue.

This colour is in the greatest perfection, when the herb is young, and contains the most juice, the thinnest and most colourless part of which produces the best blue: and it will be found by the experiment, that when the stalk is very forcibly squeezed, the paper is coloured rather green at first, and never becomes of a good colour. On the contrary; the place touched with the clearest and thinnest part of the juice, discovers no colour for many hours, but at last exhibits the finest blue contained in the vegetable. This colour is not very durable, but after some time turns to a red, which will continue for years.

A friend of mine, Mr. R. P. to whom I communicated these properties of the juice, and requested him to devise some experiments, introduced some of the leaves and stalks of the herb, into a small phial containing a solution of prepared ammonia, and in a little time the solution was coloured blue.

The herb, when dry, exhibits no particular appearance; but the roots, which are long, slender, and white, when dried, discover in some places a fine blue tinge upon the outer skin; and if a quantity be steeped in water, it becomes of a blue colour, which afterwards turns red.

A solution of ammonia extracts the colour much sooner than water alone, and, I think, improves it.

Some years ago, I discovered that the juice would stain a blue colour when laid upon a piece of white paper or fine white cloth; previous to that, however, I understood, that certain individuals had endeavoured to procure a body of colour from this vegetable; but of their success, or the methods of conducting their processes, I could learn nothing with certainty.

The properties of this vegetable appear singular to me, and, if they are not generally known, may perhaps deserve the attention of your philosophical readers, as the elucidation of the subject seems likely to be of some advantage to the arts.

It is proper to be observed, that as the plant is of a poisonous nature, it must not be carelessly used.

I am, Sir,

Your humble servant,

C.

April 19th, 1800.

*Account*

## VII.

*Account of a Work entitled "The Observations of NEWTON concerning the Inflections of Light; accompanied by other Observations differing from his; and appearing to lead to a change of his Theory of Light and Colours." By a Correspondent.*

**T**HIS anonymous work consists of the observations of Newton concerning the inflections of light contained in his third Book of Optics, reprinted, and accompanied by new facts and observations\* made concerning the same phenomena, together with inferences, which differ altogether from those of Newton.

In his first observation, Newton, repeating an experiment of Grimaldi, perceived as he had done, that in a beam of solar light passing through a small hole  $\frac{1}{42}$  of an inch wide, the breadth of the shadow of a hair observed at a distance from the hole, was considerably greater than that of the hair itself. This he conceived could only be effected by a *repulsive* power in the hair upon the *parallel* rays of the beam, turning the light aside from the shadow, and rendering the shadow considerably *broader than it ought to be*, if the rays passed by in right lines. By actual measurement he further ascertained, that the shadow was broader in proportion to the distance from the hair when nearer, than when farther from it; and this he imagined to depend upon the diminution of the repulsive power upon the more distant rays of light, in consequence of which they were less turned aside from, and therefore in their progress invaded and reduced the dimensions of the shadow.

This of Newton is followed by an observation marked  $\alpha$ , of the same appearances. In which the first thing noticed is, that the beam of light after passage through the small hole, is no longer a beam of parallel rays, as Newton supposed, and has drawn it in his figure, but consists of divergent rays. This is proved to be the case by actual measurement, due allowance being made for the penumbral light of the sun, and is stated to appear more clearly by immediate inspection of the various changes produced in the beam. This divergence is ascribed to the well established principle of an attractive force exercised by bodies upon light, in consequence of which when the hole is very small, its edges act upon the whole of the passing light, and render it altogether divergent.

In such divergent light, therefore, the shadows of bodies ought to be of dimensions considerably greater than the bodies themselves, in proportion to the divergence of the rays, and to the distance of observation. This proportion, however, is not exactly preserved, because the rays in passing by the hair, undergo from the *attraction* of the hair a change of direction *towards* the shadow, in consequence of which the shadow is really *less broad*, than it would have been if the rays had passed in right lines, and this proportion of the shadow is greater at smaller, than at larger distances. These things are clearly shewn by a figure,

\* The observations of the author are designated by the letters  $\alpha$ ,  $\beta$ ,  $\gamma$ ,  $\delta$ , &c. of the Greek alphabet.---N.  
and



and the principle of attraction is so well established, as to leave no doubt of its affording the proper explanation of these appearances.

In opposition to the authority of Newton, he is quoted against himself. Five passages are produced from his works, which prove, that on different occasions he reasoned inconsistently and contradictorily concerning these phenomena.

This observation is concluded with the curious remark, that strictly speaking, all this reasoning has been applied, to what in fact is not a shadow, formed by the interception of light by the body, as appears in a subsequent observation  $\gamma$ , but a dark interval of the nature of those observed between the fringes of colours afterwards mentioned. So little have these phenomena been understood!

The second and third observations of Newton next follow, and are succeeded by the author's observation  $\beta$ , in which, instead of only three noticed by Newton at the termination of the shadow of the hair, an uninterrupted succession of fringes are described, the colours of which are stated to vary somewhat from those, the fringes themselves admitting of various changes, and this new arrangement being made with a view to analogies, that are stated to exist between these and other similar appearances, and in order that they may generally accord therewith, and with other observations of Newton made elsewhere.

Newton's fourth observation describes the successive appearance of the three fringes, with their dark intervals at different distances from the hair, and concludes from measures taken, that the squares of the breadths of the fringes, and their intervals together, are in the continual progression  $1, \frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}$ , or thereabouts. Observation  $\gamma$ , states, that at a very small distance from the hair, the shadow appears distinct, well-defined, and intensely black; that at a greater distance it is divided through its whole length by a line of light, and changed to a double shadow, resembling the shadows of two hairs, but not so intensely black as before; and that at yet greater distances the shadow increases in breadth, and diminishes in blackness, whilst the line of light putting on colours, and becoming more and more dilute, spreads over and disappears within the shadow, which again loses its double appearance. The first only is a proper shadow, which is quickly invaded and destroyed by the inflected lights of the two nearest fringes on both sides of the hair, which meet together at that distance therefrom, and the apparent double shadow consists of the two succeeding dark intervals, which become single as the intermediate light becomes dilute, and ceases to appear. This is what has been called a shadow by Grimaldi and Newton, and has been considered and measured as such.

The fringes and their intervals, as they are formed under various circumstances, and their various dimensions are then shortly noticed, and it is affirmed, contrary to the observation of Newton, that the breadths of the fringes and intervals, so far from observing generally the proportions assigned by him, do vary indefinitely through almost all degrees of their possible extension.

According to the fifth observation of Newton, a beam of light passing through a hole  $\frac{1}{8}$  of an inch wide by the edge of a knife, on the blade of which part of the light fell, exhibited



exhibited *two* streams of faint light shooting out both ways from the beam into the shadow like the tails of comets, at an angle of about ten or twelve degrees, and sometimes farther; at the same time that a line of light appeared at the edge of the knife, visible even out of the direction of the general light from parts beyond either the point or handle of the knife. This light, Newton observes, was *contiguous* to the edge, and narrower than the light of the *innermost* fringe, and passed *between* it and the knife. The author, in his observation  $\delta$ , remarks, that the preceding circumstances constitute only one of the many cases of the inflexions of light in which no fringes are formed, but only a general derivation and divergence of the rays is produced, which is owing to the parallelism of the rays of light not being changed by their passage through the large hole, but wherever the diverging of a beam of light is such that the inflexions of a body can give them the due arrangement, fringes are formed of endless varieties as these circumstances vary. Three cases of fringes, making, with the former, four general cases of inflexions, are then stated as including these varieties of appearance as the dispersion of the beam from its greatest, is diminished to its smallest state of divergency and to parallelism, and it is explained and shown by figures how the formation of fringes depends upon the divergency, and is destroyed by the parallelism of a beam. The Newtonian experiment is then repeated with a beam of light passed through a hole quarter of an inch wide, in a plate of lead, the *two* streams of light observed by him concerning the origin and existence of which much doubt and difficulty had arisen among philosophers, are shown to be owing the one to the edge of the lead of the hole, the other to the edge of the knife, and to be produced in the parallel beam of light. The line of light observed by Newton is noticed as indeed worthy of particular attention, being in a very remarkable manner derived out of the whole body of the light passing by the edge, and not merely from that *contiguous* to it, and not being narrower than the *inmost* fringe, or passing *within* it, because in this case no fringes are or can be formed, passing, however, at those very points near the body at which, under other circumstances, the *many* nearest fringes are formed.

According to observations the sixth and seventh, by approaching the parallel edge of another knife to that of the former, at the distance of the 400th part of an inch, just before they touch, the whole passing light is divided into two streams, each bent towards the nearest edge with a dark shadow between them, which streams vanish upon contact of the knives, their parts *farthest* from the direct light vanishing *last*. As the knives, however, approached each other, and before the shadow appeared, the three fringes themselves appeared on the inner ends of the streams on either side of the direct light, and grew distincter and larger until they *all* vanished, the outmost first, the middlemost next, and inmost last, leaving the streams of light described in the fifth and sixth observations. These streams, therefore, Newton concluded passed by the edges at *less* distances than any of the fringes, and the fringes at different distances according to the order of their vanishing.

Observation

Observation  $\epsilon$  distinctly states all the phenomena that occur at the parallel edges of two knives in a beam of parallel light approaching towards each other in the same plane. At first appear two borders of inflected light, one at each edge, similar each to the one before observed at the single edge. In these borders, and of this light, are made two sets of fringes, one at each edge, which beginning in the form of slender white streaks and dark intervals on the edges of the borders nearest to the knives become broader, and coloured, and by degrees occupy the whole light between the edges, forming two complete sets of fringes, each composed of the colours before described in observation  $\beta$ , and beginning in the centre of the passing light. These fringes grow broader, then vanish by pairs, until at last only one pair, with a dark interval between them, remains, which decrease and vanish upon the contact of the knives. From this statement it appears, that Newton was mistaken in considering this last pair of fringes as streams of light distinct from the fringes, and as being the very borders themselves of inflected light observed at the edge of a single knife, out of which they are indeed formed, and of which they really are only an inconsiderable part. In this case each edge by inflecting gives the light passing nearest to it a due divergency, and the other forms fringes therein.

In observation  $\zeta$  a very small hole,  $\frac{1}{50}$  of an inch wide, being used, gives a due divergency to the light, and, therefore, the two edges, when even at a distance, form fringes to themselves of an arrangement of colours, the inverse of those in the preceding observation instead of borders of white light, but upon their nearer approach break up and dissolve these, and by their more powerful mutual attractions form fringes in a contrary order, similar to those of observation  $\epsilon$ , thus exhibiting, under different circumstances, two different sets of fringes.

Observation  $\eta$ , shews that even in different distant planes the same phenomena, with due change of circumstances, are produced at the edges of two knives.

Observation  $\vartheta$ , by placing parallel sided plates of lead, of different breadths, in a divergent beam of light passed through a very small hole, produced not only the two usual sets of fringes at their external edges, but also a second double set within the shadow beginning from the centre thereof, both sets of fringes growing broader as the breadth of the lead was diminished, and the fringes in the shadow vanishing by pairs, and at last entirely disappearing. These fringes are the coloured streaks observed by others on the shadows of small bodies. Cylinders also of successively reduced diameters down to the hair of the first observation, produce the same appearances, and even with the same cylinder or plate of a due size, the same appearances may be produced by distance or obliquity of observation.

In Newton's eighth observation the edges of the knives being inclined until they met, and the beam passing through a hole  $\frac{1}{42}$  of an inch wide, at a very small distance from the knives the fringes appeared at, ran along the edges and met, and where they met ended; but at a much greater distance they appeared to grow broader as they approached, then met and crossed each other, and then appeared broader than before, whence he concluded,



that by the approach of the edges the distances of the fringes therefrom are not *increased* or *changed*, but that the bendings of the rays are thereby *increased*, and that the nearest knife determines which way the ray shall be bent, and the other *increases* the bent. Observation 1, repeats the experiment, and shows how by the inclination of the edges all the appearances which at different times were before produced by the successive approach of the edges, are now all at one time exhibited at their different distances from each other. The fringes where the edges are distant are formed in the light rendered divergent by the small hole, where they appear to have crossed they are made by each remote edge in the light bent by the nearest edge, in light which, if the remote edge was absent, would there form the fringes of the nearest edge; upon the approach, therefore, of the knives, the distances of the fringes of each knife *are changed* and increased, contrary to what Newton observes, for they are then made in light beyond that of the first fringes of each edge, that light nearest to each edge composing *then* the fringes of the remoter edge, and as each edge thus makes its fringes in the light bent by the opposite edge, the nearer it approaches in more bent light will the fringes be made; not that the approach bends the light, it lessens the bent by contrary attraction, but changing at the same time the light of the fringes, it makes them in other light nearer to the bending edge, and, therefore, more bent than the former light, and, therefore, the fringes are also broader. This making of broader fringes in light more bent led Newton into a wrong conclusion. The light does not increase its bending and divergence, the fringes change their light: by the approach of the edges they are made in light more bent and diverging, but the general bent of the rays is really *diminished* by that approach, and whilst the nearest knife continues to bend the rays towards itself, the other knife *diminishes* that bent.

In observation the ninth, Newton having measured at different distances from the knives the dimensions of the fringes, intervals, and shadows, and finding that the distances of the point of crossing of the dark lines between the first and second fringes from that of the concurrence of the shadows of the edges, continually increased, and taking it for granted, contrary to the fact, that these gave the true measures of the distances of passage of the light itself from the concurrence of the edges, concluded, that the light which made the fringes was *not the same* light at all those distances, but passed *nearer* to the knives, and was *more* bent when the fringes were observed at small than at greater instances. Observation 2, shows that these increased dimensions are merely produced by the divergency of the light of the experiment, a circumstance which escaped Newton in his first observation also, and that, consequently, the fringes are always *the same* at all distances of observation, and are made in the *same* light *equally* bent, and passing at the *same* distance from the edge.

Observation the tenth of Newton is employed in an attempt by actual measurement to ascertain the nature of the curves formed by the fringes and intervals in the preceding experiment. They are determined to be hyperbolic, and their asymptotes drawn. The fringes themselves are by him separated, and distinguished from another triangular light,



supposed to appear between the two last nearest the concurrence of the edges; but observation  $\lambda$ , remarks, that this light is really the light of the last pair of fringes nearest to the concurrence of the edges exhibiting the appearance and colours which should belong to these fringes under such circumstances, and from the oversight discovered with respect to this pair of fringes, from the accuracy required in the measurements of these shadowy parts, from the actual parallelism of the lines of the fringes on one side to, and the apparent probability that they will cross and cut the asymptotic lines on the other side, questions the accuracy of the determinations respecting these lines. Observation  $\mu$ , shows how by using a triangular piece or cone of lead, or the points of pins of various sizes, instead of the parallel-sided pieces or cylinders of observation  $\beta$ , all the appearances of that observation are at once produced at the edges of the triangle or cone.

In observation eleventh, Newton examines the fringes made by differently coloured prismatic lights, and finding the fringes made by red light alone formed at greatest, by violet at smallest, and by intermediate colours at intermediate distances, as if all those lights were mixed in the white, and early separated at the time of inflexion, concludes that the colours arise not from any modifications impressed upon the rays of light by the hair, but from simple separation by various inflexions of the several sorts of rays. To this mode of explanation another is opposed in observation  $\nu$ , namely, that the different rays when apart are, indeed, inflected in the same manner to make fringes of various colours, as are rays of the same colour when being at once variously separated from white light they make, together with the rest of the colours, party-coloured fringes. This, however, is attributed not to simple separation alone, but to modification at the time of separation, which gives to each portion of the white light thus separated a permanent condition, distinct from all the separated portions, and the original portion out of which they were separated.

Separating by inflection a small portion of white light into various colours, and then returning the various rays back again to the same point under the same circumstances, or supposing them so returned, they will unite into the original portion, or supposing them returned without other change to different points, they will move in parallel lines to each other, preserving their distinct characters; or if now a second time returned without further change, coming in the same parallel direction, they will be inflected differently. These circumstances evince a change and permanent constitution, superinduced upon each of these portions, and by which they are rendered distinct from each other, and from the original portion until their re-union into the same, as a portion similar to that from which they were derived. These small portions of white light are its rays, which pass through the intervals between the particles of bodies, and coloured rays are always formed therefrom by similar modifications, produced by the actions of the particles of other bodies at the same time, modifying as well as separating them. The very mode of distribution of the white light argues the action of a principle, modifying as well as separating, rather

than the existence of different sorts of rays, which may not easily be supposed so numerous, and capable of so regular a dispersion. Thus modification is established, as well as separation, and these principles of condition and formation alone suit the phenomena. The fringes before observed, require complete sets of the different coloured rays to issue *at the same time*, from *each interval* or point of passage of the medium. For this purpose an appropriate arrangement is required of the rays; if of originally different sorts in different passages, and only mixed in the white light, lest a nearer red should be more inflected than a further blue, and the like, and this arrangement will even then agree with only one station of the inflecting body; but this station is indifferent in producing the phenomena. A confused mixture therefore, of rays, is inconsistent with the phenomena, no collateral arrangement will answer; and if all the different rays must be considered as passing through all the intervals of the medium, they cannot all issue from, or be returned at the *same time*, to the *same place* as the phenomena require. By supposing, however, that the time of succession of all the rays in the same passages is imperceptible to sense, then the hypothesis of simple separation, and original distinctness of the rays, may answer for these phenomena, as well as that of separation and modification; but as they will both then answer equally well, other the general phenomena can alone determine which is true, and by them they must be judged.

Omitting, however, all consideration of these two hypotheses, it is clear, that in the preceding observations concerning inflections, Newton is mistaken as to all the most important points. He has upon different occasions, endeavoured to establish a power altogether repulsive, or a power changed at different distances from attractive to repulsive. This repulsive power has been always hitherto used in philosophy, to account for the reflections of light, and as it is clearly shewn not to exist, another manner of accounting for reflection is to be sought for.

The concluding observations of the work are general. It is stated among other things, that the Newtonian doctrine of light and colours is unfounded as to both its principles; the constitution of solar light (questioned indeed in the last observation), and the fits of easy transmission and reflection of the rays. The same Author is preparing to publish, *Observations concerning the Colours of thin Plates*, shewing the phenomena to be merely inflections of light, that the Newtonian fits of easy transmission and reflection have no existence, and that Newton's doctrine of the grounds of the colours of natural bodies is unfounded.

The observations of Newton in the above work marked with the numerical figures, are printed with a reduced margin; the new observations marked with the letters of the Greek alphabet, are printed with a full margin.



## VIII.

*Singular Instance of Fecundity. In a Letter from the Rev. W. PEARSON of Lincoln.*

To. Mr. NICHOLSON.

SIR,

I Was lately informed by Mr. Morris of Dunham, in this county, that he has an ewe which on Friday night, and Saturday morning last, yeaned *five* lambs all full grown; which I consider as so remarkable an instance of fecundity, that I am induced to transmit a notice of it for public record in your Journal.—The ewe is somewhat above a middle size, and is a *cross* of the Lincolnshire and Leicestershire breeds, three years old, and the ram which she was put to, is of a thorough Leicestershire stock: three of the lambs were male, and two female; they are all dead, but four of the five were found alive, and it is supposed would have survived, if the inclemency of the night had not starved them. The shepherd, who vouches for the veracity of these facts, asserts, that the fourth lamb came in an unnatural position, and that the ewe when he first went to her about midnight, was turned upon her back, and must have perished, on account of being in that position, if he had not accidentally come to her assistance: she is, however, perfectly recovered, and many circumstances concur in proving indubitably, that, though there were about thirty other ewes in the same grounds at the time, yet the five lambs in question were all yeaned by the same ewe: I will mention one proof, which of itself may satisfy those readers, who may otherwise feel disposed to doubt the authenticity of my information:—the shepherd, upon a supposition that the two first lambs that came were all that he had to expect, took them into a shelter, and returned to take care of the ewe, when, to his surprise, he was presented with two more; these were taken away also, and on his second return, the fifth was found dead newly yeaned, and no other sheep was near:—the shepherd is not guilty of fabricating tales, and Mr. Morris, who is a respectable yeoman, has at this time all the five skins, which were taken from the lambs.

It may not be improper to add, that the other ewes of the same stock, and in the same pasture, have some two lambs, and some only one each, with the exception of one which has three living.

I am, Sir,

Your obliged Correspondent,

Lincoln, April 14, 1800.

W. PEARSON.

P. S. Since the receipt of the above, the learned writer has favored me with an extract from the Lincoln, Rutland, and Stamford Mercury of 18th inst, stating that “on the 24th ult. at Mardon Woole, in Northumberland, an ewe, the property of T. Reed, Esq. yeaned



yeaned five lambs, four of which were then living, the other appeared to be something of an embryo foetus, though nearly perfect;" and that "what tends to heighten astonishment, is, that the same ewe has had an equal number at each time for two preceding years."—W. N.

## IX.

*On the Phenomena of the Morning and Evening Dew. By C. A. PRIEUR\*.*

THE evening and morning dews (*le Serein et la rosée* †) present themselves so often to our observation, and in so many different situations, that it is surprising that philosophers should have attended so little to these phenomena, or that they should have been contented with very loose explanations on this subject.

In the year 1788, I had occasion to ride out very frequently on horseback in the morning and the evening, and being thus exposed to the impressions of the dews, I was induced to meditate particularly on these appearances, hitherto so little examined. I well knew that the humidity deposited on bodies in the open air at the setting of the sun, is not the same as is afterwards observed upon them at its rising; and that consequently there is an interruption in the appearance, namely, an evaporation of the evening dew, or humidity, and a new production of moisture in the morning. I was also aware of that partial explanation of the evening dew, by which it is said, that the diminution of the heat in the air renders it incapable of retaining any longer the water it had taken up during the day. But why is there a wind always blowing from that side of the horizon occupied by the sun, and constantly accompanying this precipitation of water; and again, how does it happen that the same luminary a short time before its rise, and even after having given a slight degree of warmth to the air by its presence, appears to cause a greater cold, a stronger wind, and a more abundant precipitation of water than in the evening.

To remove this difficulty, I had recourse to the fundamental principles, by the assistance of which, Monge has so ingeniously accounted for the greater part of the phenomena of meteorology‡; and I soon perceived, that they were sufficient to explain the appearances which formed the subject of my enquiry.

These principles, which it will be proper to enumerate, are the three following; 1. The air in like circumstances dissolves more water the greater its density, that is to say, the more it is mechanically compressed.

2. And also more the higher its temperature.

\* From a letter addressed to Cit. Hassenfratz, and inserted in the Journal of the Polytechnic School, vol. ii. cahier 6. p. 409.

† We have not distinct words corresponding with these in our language?—N.

‡ Annales de Chimie, tome V.

3. Under equal pressure, and at like temperatures, air holding water in solution has a less specific gravity than air alone; and this specific gravity is less the greater the proportion of water so dissolved.

It must also be recollected, that the changes of pressure and weight which may take place in certain columns of the atmosphere, must necessarily disturb the equilibrium, and produce motion or currents in the same.

These principles being established, let us suppose for a moment, to render our inquiry more simple and easy, that the earth deprived of its rotatory motion, shall remain motionless in the presence of the sun, and let us besides dismiss from our consideration all local influence, and in general all those causes which might disturb the regularity of the effects.

In this state of things, what will be the consequence with regard to our atmosphere?

The air exposed to the rays of the sun will be heated, and principally in the part contiguous to the earth, on account of its greater density, and the reverberation of that planet. This heated air will acquire a greater dissolving power, and will, in fact, dissolve much water if that fluid be present. It will take it from the seas, lakes, rivers, ponds, and other reservoirs, whose surface is exposed to its action, and will even absorb a portion of the humidity of the ground.

Let us in the next place consider, what happens in any assumed vertical column in the atmosphere, and first in that placed immediately beneath the sun. The air heated at the lower part dissolving water, and thus becoming specifically lighter, will rise, and be replaced by other contiguous air. An ascending current will therefore be established in the column. If this column were separated from the others, as if it were contained in a vertical tube, in proportion as the lower air rose loaded with water the upper air would descend, become charged in its turn, rise and be replaced; and that perpetually. And the air holding water in solution, would in consequence of under saturation from cold and diminished pressure, when it had arrived at a certain height, let fall the excess of water, and form a mist or cloud, which might continue to rise by virtue of its acquired motion, but which, after a certain accumulation, would fall again in rain. It may also be conceived, that this translation of the air upwards and downwards, would be made either by a mutual infiltration, or currents in both directions, which would be constantly and regularly kept up.

But this is not entirely the case with our vertical column, because it is not in fact separated from the others in its vicinity. These also are subjected to the same operations, with the exception only, that the effects are less the more remote they are from the column immediately beneath the sun. The heat, the solution of water, and the force of ascension, gradually diminish as the distance from this central column increases. If therefore the surface of the earth were a plane, the proper representation of the rising air would be a cone with its summit directed to the sun; and on the other hand, as the absolute weight of each column is increased by the whole of the water it has dissolved, the equilibrium requires that

that there should be on all sides a lateral divergence, which must evidently take place where the pressure is least. Thus on the same supposition of the earth being a plane, we should observe the upper air descend and precipitate itself along the sides of the cone, producing by this oblique direction to the axis, a current on all sides directed from the sun; and this current would be increased by the vacuum formed at those places where the air is sufficiently elevated, and could precipitate its superabundant water.

This image will require very little more dification to apply it to the exterior of our globe. The conical surface will be converted into a cap, enveloping the enlightened portion of the earth, and following the curvature produced by the currents.

Inspection alone of the figure in Plate 3, will shew this curvature. The globe may be readily seen placed in the center of a circular stratum, supposed to be filled by the atmosphere. The circumference of the earth has twenty-four divisions, from which are raised a like number of perpendiculars to its surface, or vertical lines, in order to afford a notion of the change of phenomena from hour to hour. The sun supposed to be placed at S, in the continuation of the line TS, is supposed to have all its rays united in the center; and lastly, the curve *a, b, c*, though arbitrarily drawn, because its law is unknown, will be sufficient to shew the existence of the effects which we are interested to explain.

Such, therefore, will be the result of the supposed circumstances. Water dissolved by the lower air exposed to the sun; a motion of ascent in this part; descending divergent currents on all sides, spreading over the earth. These currents, moreover, communicate their water to the heated columns, which rise and diverge laterally; and this water is precipitated, because the air of the currents proceeding from the upper regions is too cold to keep it in solution, or because the pressure is less from the diminished weight of the columns, as their distance from the line immediately beneath the sun is greater, as well as because they may mix with other cold air near the surface; and lastly, the ground, and all other bodies in the direction of this precipitated water, will be wetted therewith.

The morning and evening dews are here evident to our consideration, with the wind and the cold which attend them; but a few more remarks are wanting to complete the description. We must first observe that the descending currents are prolonged till the resistance of the air, through which they pass, has entirely destroyed their motion. Again, we shall see immediately beneath the sun, a circular space greatly heated, which does not present to the inhabitants of that region the phenomenon we have been describing. As we depart from this space, and in proportion as the sun appears in a more oblique direction, we arrive at a region less heated, where the wind from the direction of the sun, and the precipitation of moisture, begins to be perceived. This region forms a crown round the circular space before mentioned. And, lastly, by departing still more from the inner border of this crown, the wind is found to be stronger, the cold more perceptible, and the precipitation of water more abundant; which effects afterwards diminish to a certain distance, and entirely cease at the surface of the earth diametrically opposite the sun.



In this manner, on the hypothesis of the immobility of the earth with regard to the sun, there would be beneath that luminary a very extended region eternally, and uninterruptedly subjected to the phenomenon of the morning and evening dew, accordingly as the observer was placed to the east or the west.

But let us now assume the real state of things, and restore to the earth its diurnal rotation. The preceding phenomena will then take place on successive parts of its surface. Those places from which the sun is descending, and ready to set beneath the horizon, will soon perceive the appearance of the evening dew with a west wind springing up, which phenomena will increase gradually till after the setting of the sun, and then the effect will diminish and entirely cease. During the night the humidity will evaporate, and entirely disappear, provided the air be not already too much loaded with moisture. Towards the next morning, just before day-break, the phenomenon will again present itself on the eastern side, with the same circumstances and gradations, the maximum of effect being also when the sun is yet beneath the horizon; but with this very remarkable difference, that the effects will be much stronger than those of the evening; that is, there will be more wind, more moisture deposited, and a more sensible degree of cold. The reason of this is, that in the evening the precipitation of water, the wind, and the cold which accompany it, ought to be diminished, because the whole takes place in the vicinity, and by the mixture of air which the sun has heated during the day; whereas in the morning the coldness of the night air permits, or gives a much greater effect to the phenomenon.

We see likewise, that in the two temperate zones, where the winter and summer have a great difference of temperature, where the length of days and nights vary much, the effects of the evening and morning dews are varied and irregular. In summer, if on the one hand the air dissolves more water in the day, on the other hand, the precipitation of the evening dew is made in an air very much heated, and that of the morning dew in air which the shortness of the night has cooled only to a certain point. In this case the solution of moisture is considerable, and the precipitation little. In winter, on the contrary, the cause of solution is less, but that of precipitation more effectual. Local circumstances, more especially the vicinity of water, must also influence the effects. Fine weather increases, and renders them more sensible. Close weather weakens or destroys them.

Under the torrid zone the days and nights are more nearly equal, and at the equator they are equal at all times. The difference between the summer and winter temperatures is less considerable than in our latitude, and the sky is almost constantly clear. It must follow, that the morning and evening dews will be heavier under this burning climate, than elsewhere on the earth. And this agrees with the testimony of travellers. In Egypt, in Asian Turkey, at the Antilles, at Mexico, in vessels sailing between the Tropics, the morning and evening dews fall so abundantly, that they produce the same effect as showers of rain.

But there is a very important consequence, which it seems allowable to draw from these phenomena; namely, that they must influence the production and permanence of the trade winds. For every day, almost regularly, the air of the torrid zone being solicited to move in two opposite directions by forces very different in quantity, it must tend in fact to acquire and preserve a motion or current, in the direction of that strongest power, which in this case is from the east, being the wind which brings the morning dew. This cause must be the more effectual, as it acts in the lower part of the atmosphere, where we feel the trade winds, and because it affects the densest portion, and consequently must the more readily move the whole.

Under the glacial zone, where particularly during the winter the sun scarcely skims the horizon through the whole day, the precipitation of water will be very considerable, by reason of the coldness of the climate, and a thick fog will prevail, which will scarcely be dissipated in summer; but in winter will extend far into the temperate zones. Thus we see during the winter season, in our country, very considerable fogs, which have only a feeble light even in the middle of the day.

It would be curious to present in this place a numerous series of accurate observations, on the circumstances which accompany the morning and evening dews at different times of the year, and in different countries, the hours at which these phenomena begin and end, the intermissions or irregularities, with which they are affected in different situations; but if the generality of the causes to which they are here attributed, should excite the attention of philosophers, the interesting task here indicated will assuredly and speedily be performed.

## X.

*Thoughts on Magnetism, by RICHARD KIRWAN, Esq; L. L. D. F. R. S. and M. R. I. A\*.*

1. **T**HERE are two ways of explaining a natural phenomenon; the first, is by discovering the conditions and circumstances of its production and the laws by which its action is governed; the second, is by shewing its analogy, similarity, or coincidence with some general fact with whose laws and existence we are already acquainted; this last mode is by far the most perfect and satisfactory. In the first sense of the word electricity and magnetism have been in some measure explained, but in the last sense neither; the primary cause of magnetism in particular has hitherto been supposed to relate to iron alone, or its ores, and to stand unconnected with all other natural phenomena.

2. If therefore any other general fact or power can be discovered to which it bears some analogy or similarity, it may so far be said to be explained. Now such fact or power I think may be assigned, namely, the power of *crystallization*.

\* Transactions of the Royal Irish Academy. Vol. VI.



3. By crystalization I understand that power by which the integrant particles of any solid possessing sufficient liberty of motion unite to each other; not indiscriminately and confusedly, but according to a peculiar uniform arrangement, so as to exhibit in its last and most perfect stage regular and determinate forms.

4. This power is now known to be possessed by all solid mineral substances.

5. The forms which crystals, even of homogeneous substances, exhibit, are often very numerous; however in most cases they may be reduced to a few primordial forms, which, as Abbé Haüy has lately experimentally proved, are derived from certain original forms appertaining to the minutest particles of their concretion.

6. The assemblage of these ultimate particles into visible aggregates, similarly arranged, necessarily requires that one of their surfaces should be attractive of that particular surface of the other, which presents a corresponding angle, and repulsive of that which presents a different angle, otherwise the various regular rhomboidal and other polygon prisms and pyramids, which crystals present us, could never exist; consequently the minutest prism, being once formed, could never be prolonged if one end of such prisms were not attractive, and the other repulsive of the same given surface.

7. Hence it has been observed that crystalization never takes place in the middle of any solution, but always begins at the surface or on the bottom or sides of the vessels that contain it, for the particles in the middle of the solution being confusedly mixed with each other, and exerting their repulsive as freely as their attractive powers, the one constantly counteracting the other, no sensible accretion of a regular kind could take place, whereas the repulsive power of the uppermost particles, or of those that rest on the sides or bottom of the vessel, is restrained and impeded.

8. The repulsive power of crystalizing substances also appears in many other instances (of the attractive no doubt has ever been formed.) Thus if saturate solutions of nitre, common salt, and tartar vitriolate be mixed and set to crystalize, each will crystalize a part, which could not happen if the particles of each of these salts did not only attract their similar homogeneous, but also repel those of a different species, otherwise the mere casual circumstance of greater proximity to one than to the other would impel them to unite indiscriminately. Again, if a saturate solution of allum be mixed with a turbid mixture of clay, and abandoned to insensible evaporation, after some time the clay will subside and form a dry mass, but in the interior of this mass large regular crystals of allum will be found, whose component particles must, to reunite, have displaced and repelled the particles of clay with which they were surrounded.

9. If to a saturate solution of a salt that difficultly crystalizes, a crystal of a salt of the same species be inserted, the whole solution will soon be brought to crystalize, as the crystal inserted attracts the particles dissolved, by its different surfaces; but if a salt of a different nature be inserted this will not happen, crystalization will not be promoted.

10. If to a solution of 2 parts nitre, and 3 parts Glauber's salt in 5 parts water, a crystal of nitre be inserted, the nitre alone will crystalize; or if instead of nitre a crystal of Glauber



be inserted into it, the Glauber alone will crystalize, Do not these experiments fully evince both the attractive and repulsive powers, not only of different salts but of different surfaces of the same salt?

11. These powers within their proper sphere of action have been found indefinitely great; thus water confined in cannon several inches thick, and exposed to a degree of cold much beneath the freezing point, has been observed to crystalize into ice that burst the metallic impediment opposed to the form it then assumes.

12. The vast difference however attending the developement of these two powers (of magnetism and crystalization) will undoubtedly strike many as an insuperable objection to their identity, yet their *direction* in all its varieties being exactly the same, difference in other circumstances seems to me to indicate rather a variety of *degrees*, in the same power, than any essential difference in the powers themselves.

I now come to the application of the above principles to the magnetic phenomena. These may in general be reduced to the following, viz. *Attraction, Repulsion, Polarity*:

*Communication.*

*Declination.*

*Inclination.*

*Exclusive appropriation to Iron.*

*Destruction of the Magnetic power.*

#### 1st, *Attraction, Repulsion, Polarity.*

The quantity of iron found on and within such parts of the surface of the globe as we are acquainted with, far surpasses that of any other mineral substance singly taken, or even of many of them taken together; scarce any stone or metallic ore or earth is found free from it; it enters into their composition in the proportion of from 2 to 18 or 20 per cent. and perhaps at a medium we may state it in all of them at 6 per cent.; moreover its own ores are of all others the most common and the most copious; in many places, particularly in the most northern climates, whole mountains of it are found, and many of them magnetic. When to this consideration we add that of the specific gravity of the globe, which has been found to be 4,5 times heavier than water, notwithstanding the immense quantity of water that covers the greater part of its surface to considerable unknown depths, and notwithstanding that the specific gravity of by far the greater part of the stones and earths it contains does not exceed and scarcely amounts even to three times the weight of an equal bulk of water, and that the quantity of mineral substances whose specific weight exceeds four times that of water is almost infinitely small in comparison to the other known component parts of the globe, and finally that the weight of most iron ores is about four or five times that of water; all this I say considered, it is difficult to avoid concluding that the interior part of the globe consists chiefly of iron ore disposed in one or more aggregate masses; a conclusion that is farther confirmed, on reflecting that volcanic lavas ejected from

from the deepest recesses with which we are acquainted contain from 15 to 20 or 25 per cent. of iron in the state most favourable to magnetic attraction.

Taking then this assertion to be as fully proved as its subject matter is capable of being ascertained, we may deduce from it the following corollaries :

1st. That as the ferruginous matter in the globe being by far the most copious, its universal attractive power is principally seated in the ferruginous part.

2d. That as all terraqueous matter was originally in a soft state, its parts were at liberty to arrange themselves according to the laws of their mutual attraction, and in fact did coalesce and crystalize in the direction in which they were least impeded by the rotatory motion of the globe, namely in that which extends from North to South, and principally and most perfectly in the parts least agitated by that motion, namely those next the centre.

3d. That this crystalization, like that of salts, might have taken place in one or more separate *shoots*, or as we may here call them, immense separate masses, each having its poles distinct from those of the other, those in the same direction repulsive of and distant from each other.

In consequence then of the universal law of attraction of the particles of matter to each other, these internal magnets exert a *double* power of attraction ; the first and most general, on the particles of *all* bodies indiscriminately in proportion to their density, and the direct or inverse ratio of the squares of their distances according as those bodies are found within or without the earth's surface ; and the second, on bodies of their own species in proportion to their homogeneity, and to the correspondence of the arrangement of their integrant particles with that of the integrant particles of these internal magnets.

A magnet therefore is a mass of iron, or of iron ore, whose oxygenation does not exceed 20 per cent. or thereabouts, whose particles are arranged in a direction similar to that of the great internal central magnets of the globe. This I call the *magnetic arrangement*.

The particles of iron attract each other more forcibly than those of any other known substance. This appears by its cohesion, hardness, elasticity, and infusibility, in each of which properties, or at least in the combination of most of them, it exceeds all other known bodies.

Hence a magnet *attracts iron* when within the sphere of its action, by forcing, in virtue of its attractive power, a certain proportion of its integrant particles into a disposition and arrangement similar to that of its own. For in this case it exerts a double attractive power, that of the particles of iron to each other, which we have seen to be the greatest of all others, and that of crystalizing bodies, which we have also seen to be indefinitely great.

The crystalizing power being at once attractive and *repulsive*, according to the direction of the surfaces, (No. 6.) hence we see that one part or end of the magnet must repel that which the other has attracted, as long as the same disposition of parts remains.

The disposition of parts in a particular magnet, being similar to that which obtains in the great internal general magnet, extends in the direction of from North to South. Hence  
magnets,



magnets, when at liberty to move with a certain degree of freedom, and iron, when a sufficient number of its particles are arranged in that direction, and has sufficient liberty to conform to it, points to those poles. Hence this property is called *Polarity*.

The magnetic power is greater or lesser according to the number and homogeneity of the particles *similarly* and *magnetically* arranged. Hence small magnets may be more powerful than a larger, and hence a magnet will attract a magnetized needle at a greater distance than one not magnetized.

The magnetic power decreases in a certain ratio of the distance of the particles that exercise it. Hence it is strongest in the point of contact, and at the poles, as it is there most *unsaturated*, and weakest in the central part, which separates the two opposite poles.

When a magnet is broken into small pieces its power is nearly destroyed, because though the poles should be all of the same kind, yet the distance of each from the opposite pole is so small that their powers counteract, and consequently destroy each other.

If when a needle is attracted by the south pole of a magnet a bar of iron be placed on the north pole, the needle is still more strongly attracted, because the iron acquires also a south pole, whose force is joined to that of the magnet.

If two needles be suspended from any given pole of a magnet they will diverge, because they both acquire the same polar arrangement. If a bar of Iron be laid on that pole of the magnet, the divergence will diminish, because the next end of the iron will acquire the disposition of the opposite pole, and consequently counteract the repulsive power of the magnet.

A magnet will not transmit its power through a bar of iron if this be too long. Muschenbrouck limits their length to six feet, but this depends on the strength of the magnet.

The power of a magnet (every thing else being equal) depends on the *number of its surfaces magnetically arranged*, and the *accuracy* of that arrangement.

The arrangement is accurate when the synonymous surfaces are exactly parallel to each other, and originally conformed to and parallel with those of the great general magnet.

The magnetic attraction is strongest in the direction perpendicular to the magnetic surfaces, and weakens in proportion to the magnitude of the angle of direction with the perpendicular, and consequently is *null* when at a right angle with it. Hence the magnetic power seems concentrated at the *poles*, and the lateral powers are the weakest, as they originate only in the oblique direction of surfaces, or from surfaces inaccurately arranged.

(To be continued.)

*Description*



## XI.

*Description of the Revolving Doubler. W. N.*

AS the doubler of Bennet, to which I adapted the following machinery in the year 1788, is not yet generally noticed in elementary works, and, consequently, little known, I have thought it might be acceptable to copy the short Description and Plate from my paper in the LXXVIIIth volume of the Philosophical Transactions.

Plate IV. represents the apparatus of the doubler supported on a glass pillar  $6\frac{1}{2}$  inches long. It consists of the following parts. Two fixed plates of brass, A and C, are separately insulated and disposed in the same plane, so that a revolving plate B may pass very near them, without touching. Each of these plates is two inches in diameter; and they have adjusting pieces behind, which serve to place them accurately in the required position. D is a brass ball, likewise of two inches diameter, fixed on the extremity of an axis that carries the plate B. Besides the more essential purpose this ball is intended to answer, it is so loaded within on one side, that it serves as a counterpoise to the revolving plate, and enables the axis to remain at rest in any position. The other parts may be distinctly seen in fig. 2. The shaded parts represent metal and the white represent varnished glass. ON is a brass axis, passing through the piece M, which last sustains the plates A and C. At one extremity is the ball D already mentioned; and the other is prolonged by the addition of a glass stick, which sustains the handle L and the piece GH separately insulated. E, F, are pins rising out of the fixed plates A and C, at unequal distances from the axis. The cross-piece GH, and the piece K, lie in one plane, and have their ends armed with small pieces of harpsichord-wire, that they may perfectly touch the pins EF in certain points of the revolution. There is likewise a pin I, in the piece M, which intercepts a small wire proceeding from the revolving plate B.

The touching wires are so adjusted, by bending, that when the revolving plate B is immediately opposite the fixed plate A, the cross-piece GH connects the two fixed plates, at the same time that the wire and pin at I form a communication between the revolving plate and the ball. On the other hand, when the revolving plate is immediately opposite the fixed plate C, the ball becomes connected with this last plate, by the touching of the piece K against F; the two plates, A and B, having then no connection with any part of the apparatus. In every other position the three plates and the ball will be perfectly unconnected with each other.

Mr. Cavallo's discovery, so well explained in the last Bakerian Lecture, that the minute differences of the electrization in bodies, whether occasioned by art or nature, cannot be completely destroyed in any definite time, may be applied to explain the action of the present instrument. When the plates A and B are opposite each other, two fixed plates A and C may be considered as one mass; and the revolving plate B, together with the ball

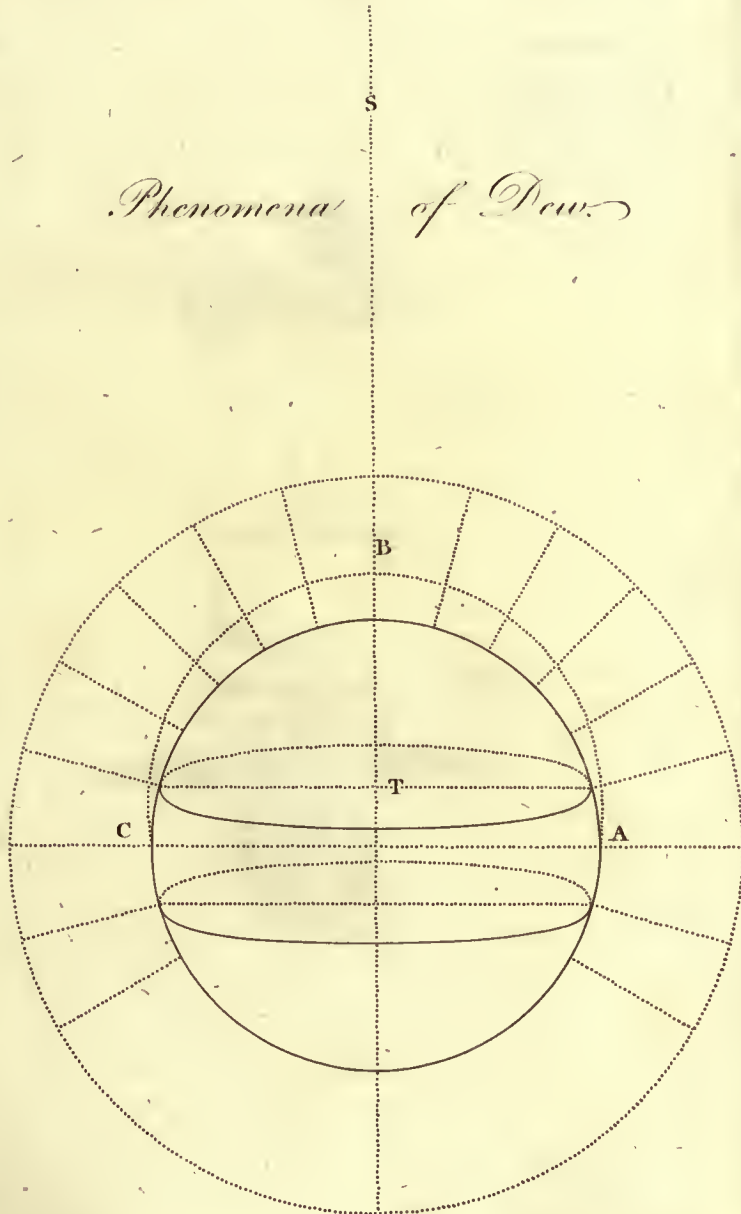
D,

D, will constitute another mass. All the experiments yet made concur to prove, that these two masses will not possess the same electric state; but that, with respect to each other, their electricities will be plus and minus. These states would be simple and without any compensation, if the masses were remote from each other; but as that is not the case, a part of the redundant electricity will take the form of a charge in the opposed plates A and B. From other experiments I find that the effect of the compensation on plates opposed to each other, at the distance of one-fortieth part of an inch, is such that they require, to produce a given intensity, at least one hundred times the quantity of electricity that would have produced it in either, singly and apart. The redundant electricities in the masses under consideration will therefore be unequally distributed: the plate A will have about ninety-nine parts, and the plate C one; and, for the same reason, the revolving plate B will have ninety-nine parts of the opposite electricity, and the ball D one. The rotation, by destroying the contacts, preserves this unequal distribution, and carries B from A to C, at the same time that the tail K connects the ball with the plate C. In this situation, the electricity in B acts upon that in C, and produces the contrary state, by virtue of the communication between C and the ball; which last must therefore acquire an electricity of the same kind with that of the revolving plate. But the rotation again destroys the contact, and restores B to its first situation opposite A. Here, if we attend to the effect of the whole revolution, we shall find that the electric states of the respective masses have been greatly increased: for the ninety-nine parts in A and in B remain, and the one part of electricity in C has been increased so as nearly to compensate ninety-nine parts of the opposite electricity in the revolving plate B, while the communication produced an equal mutation in the electricity of the ball. A second rotation will, of course, produce a proportional augmentation of these increased quantities; and a continuance of turning will soon bring the intensities to their maximum, which is limited by an explosion between the plates.

If one of the parts be connected with an electrometer, more especially that of Bennet, these effects will be very clearly seen. The spark is usually produced by a number of turns between eleven and twenty; and the electrometer is sensibly acted upon by still fewer.

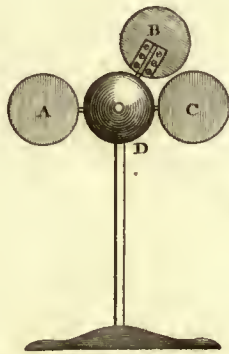
If the ball be connected with the lower part of Bennet's electrometer, and the plate A with the upper part, and any weak electricity be communicated to the electrometer, while the position of the apparatus is such that the cross-piece GH touches the two pins; a very few turns will render it perceptible. But here, as well as in the common doubler, the effect is rendered uncertain by the condition, that the communicated electricity must be strong enough to destroy and predominate over any other electricity the plates may possess. I scarcely need observe, that if this difficulty should hereafter be removed, the instrument will have great advantages as a multiplier of electricity in the facility of its use, the very speedy manner of its operation, and the unequivocal nature of its results.

*Phænomena of Dew.*

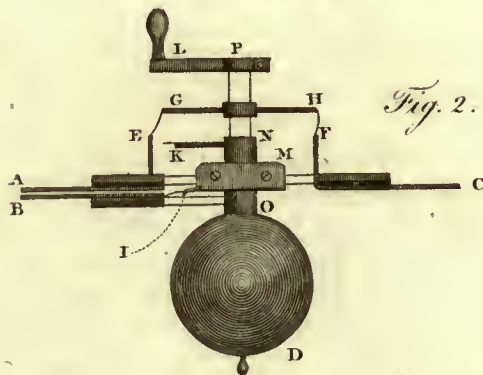








*Fig. 1.*



*Fig. 2.*





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A  
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AND  
THE ARTS.

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JUNE 1800.

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ARTICLE I.

*Observations on the Proofs of the Huttonian Theory of the Earth, adduced by Sir James Hall, Bart. By R. KIRWAN, ESQ. Communicated by the Author.*

AS some positions which I laid down in my examination of Dr. Hutton's Theory of the Earth, may seem questionable from the ingenious reasoning employed by Sir James Hall in the third volume of the Edinburgh Transactions, to corroborate some of Dr. Hutton's assertions, and may even be thought inconsistent with some of the curious results that occurred in the highly interesting experiments instituted by the worthy Baronet, and inserted \* in the fifth volume of the Edinburgh Transactions (a printed transcript of which he has had the goodness to send me), I think it a duty incumbent upon me to examine both the general reasoning employed by him, and the consequences fairly deducible from his experiments. Fanciful and groundless as the Huttonian theory seems to me to be, it may, like the researches for the philosopher's stone, be highly useful by suggesting new experiments.

In the third volume of the Edinburgh Transactions, Hist. p. 9, we are informed, that Sir James Hall, though convinced from various observations that granite had once flowed in a state of fusion, yet acknowledged that some difficulties accompanied this opinion ;

\* Also in this Journal, IV. 8. 56.

among which the most considerable appeared to him to be this, that in some cases the felspar is seen in this stone with its crystals regularly defined, whereas the quartz forms a confused and irregular mass, being moulded on the crystals of felspar, whereas if the granite were formed by fusion, the very contrary, he says, should, it would seem, be expected; felspar being very fusible, and quartz, on the contrary, highly infusible. In answer to which, he says, "that when quartz and felspar are mixed and pounded together, it is well known they may be melted without difficulty into a kind of glass, the felspar serving as a flux to the quartz, or the felspar may be considered as a menstruum in which the quartz is dissolved; and in this view we may expect by analogy phenomena similar to those of the solution of salt in water. Now it is certain that when excessive cold is applied to salt water, the water is frozen to the exclusion of the salt; why should not the same thing happen in the solution of quartz in the liquid. Felspar, when the mass is allowed to cool beneath the point of congelation of the menstruum? The felspar may crystallize separately from the quartz, as we have seen pure ice formed separately from the salt."

In this answer several particulars deserve consideration. In the first place, water (to which felspar is here assimilated) is never *regularly* crystalized when frozen by excessive refrigeration, though, indeed, vapour may; consequently, since in the present case the felspar is said to be regularly crystalized, the parity does not hold. Again, to justify the comparison of felspar acting on quartz as a menstruum, as water does upon salt, the felspar should *always* be in the larger, and quartz in the smaller proportion to each other, as water always is to salt, and this is, indeed, the commonest case even where the felspar is not regularly crystalized, yet in Switzerland this does not happen, as Mr. Höpfner attests, 4 Helvetic Magaz. p. 266, of which specimens may be met in 2 Leske Catal. English edition, p. 375, 376, No. 37, 38, 40, 41; nor in Silesia, as Gerhard remarks, 1 Grundriß Min. System, p. 404 and 405. How then could the felspar have served as a menstruum or flux to the quartz in these cases?

3dly, It is allowed by all observers, that the cases in which felspar in granite is regularly crystalized, are exceeding few; see Lentz, Emerling, Widenman, &c. Granites, in which such crystals are observed, are called *porphyraceous granites*, and from that very circumstance judged by many observers not to be ancient granites, but of modern formation; see 2 Widenman, p. 1005, in the note. An observation similar to that of Sir James Hall has also been made by Mr. Besson, in Limoges, 29 Roz. Jour. p. 89, for he discovered veins of granite in an argillite, though this schist did not border upon any granitic mass, and hence he judged it of modern formation. Citizen Dolomieu also tells us, that such instances had occurred to him in his travels, but he thinks them perfectly distinct from the granite which forms granitic mountains, 16 Journ. des Mines, p. 22. Neither was Saussure a stranger to such granitic veins; but he accounts for their origin very differently from Sir James. §. 600.

4thly,



4thly, Various attempts have been made to fuse granites, in most of which, as has already been said, felspar is the most abundant ingredient; but in almost all the finely pulverized, the quartz remained unfused, and might be distinguished by a lens; see 1 Saussure, §. 172, 173, and 174, 1 Gerh. Gesch. §. 51, and in the first part of his new Mineral System, published in 1797, p. 412, and Hacquett in 1 Crell Baytrage, p. 34, 35, &c. It is plain, then, that in all heats with which we are acquainted, the felspar cannot but in very rare cases serve as flux or a menstruum to the quartz with which it is found in granites, the full proportion of quartz, which can be rendered fusible by its other component earths, being already contained in the felspar; and, in fact, there is no analogy betwixt water acting as a menstruum on salt, and felspar acting on quartz, for water and salt are substances perfectly heterogeneous to each other, whereas felspar and quartz are both earthy substances, of which the former contains a large proportion of the latter, as essential to its composition, and is fusible only by reason of its compound nature; but if the quantity of the quartz ingredient be increased, the whole becomes infusible, as I have experienced; whereas if the proportion of salt in water be increased, still the water will be congealable if considerably cooled; moreover quartz frequently bears the impression of stones more fusible than itself, which could not happen in any possible supposition, if all had been in state of fusion.

Again, Sir James observed, that a quantity of green glass, which had been allowed to cool slowly, was found to have lost all its vitreous properties, being opaque, white, and refractory; but being again melted by a blow pipe, and suddenly cooled, it resumed its former properties, and became glass: hence he infers, that if the glass produced by the fusion of granite had been allowed to cool with sufficient slowness, it might have crystallized, producing a granite similar to the original, p. 11.

The observation on glass, here mentioned, is perfectly just, and has been often repeated; but the analogy betwixt this case, and the formation of granite from a complete fusion of its ingredients, is far from being accurate. Glass consists of a simple earth, namely, the siliceous, united to an alkali. To form this union, it is necessary that the integrant affinity of the siliceous particles to each other should yield to the chemical affinity which the alkali bears to them, and this can happen only in so high a degree of heat, as considerably lessens the affinity of the siliceous particles to each other. If, when this union is effected, the compound is considerably and *rapidly* cooled, yet the union will still continue, because the alkaline menstruum being congealed, the siliceous particles cannot move through it to reunite to each other, though their affinity to each other in a *low* temperature be greater than their affinity to an alkali, and thus they continue in that state which we call *glass*. Two experiments set this explication beyond all doubt, the first is, that if a solution of salt in water be suddenly cooled from 140 degrees above to 6 degrees below 0 of Fahrenheit, the whole will be congealed, and no separation of the salt will take place, see 8 Nov. Comment. Petropol. p. 346. This case is perfectly analogous to that of glass. The second experiment is that of Tromsdorf, 22 Ann. Chym. p. 115, where we



find the siliceous particles to have separated by long standing (8 years) from the alkaline in a solution of silicited alkali, and to have formed perfect crystals hard enough to strike fire with steel.

That the glass thus formed, being suffered to cool slowly, should be decomposed, is very natural; it is what happens when certain salts, for instance, nitre, are dissolved in water to saturation, in a boiling heat; if the water be slowly cooled, most of the nitre will crystalize and separate itself. That the siliceous earth, thus separated, should be more refractory than before, should be also expected; both because it is not repulverized (at least not stated to have been so) and because much of the alkali, which is its menstruum, evaporates, and is volatilized during the slow refrigeration. But if the heat applied be much greater than at first, it may be vitrified a second time, as more of most salts may be dissolved in a small quantity of at 212 than at 150.

But to reproduce granite from a general fusion of all its ingredients by a refrigeration ever so low, is a very different case from that we have just considered.

Granite is an aggregate stone, consisting of quartz, felspar, and mica; of these the most fusible is undoubtedly the felspar, and the quartz the least: let us then, to indulge the worthy Baronet, suppose all three in perfect fusion in a high degree of heat, and afterwards slowly cooled, and thus each (though vouched by no experiment) gradually reproduced; the quartz, with the exception of the proportion thereof, which enters into the composition of felspar and of the mica, would undoubtedly crystalize first on the smallest diminution of heat, and being congealed in a medium still in a liquid state, I do not see why it should not form regular crystals, which, nevertheless, scarce ever occur in granite, except in cavities: over this, and after a considerable interval of time, the mica should also be regularly crystalized, and last of all the felspar should coalesce and congeal (at least in the Baronet's supposition) in regular crystals; now as the crystalizations of these three species of stone take place each at a distinct portion of time, each should occupy also a distinct portion of space, the first set of crystals being lowest, the next over that, and the last uppermost, as we find to happen when salts of very different solubility, and yet in equal quantity, are dissolved and crystalized in water, or when substances of different degrees of volatility are sublimed by fire. Now among the immense masses of granite that have been observed and examined in various parts of the globe, not above half a dozen have occurred in which the three constituent parts of granite were regularly crystalized, very few in which distinct layers were seen, and *none at all* consisting of distinct regular crystals of each, superimposed on each other. On the contrary, in far the greater number of granitic masses the three above-named constituent masses lie intermixed with each other in the most confused and irregular manner, without any appearance of regular crystalization, in so much, that none can say, from bare inspection only, which was crystalized first and which last. Nay, granitic masses not unfrequently occur, in which it is evident that the mica must have crystalized contemporaneously with the quartz; for in breaking the quartz part, flakes of mica are found within it. See G. Sauff. §. 1621.

Lastly, I must add, that even on the supposition that distinct crystals of quartz, felspar, and mica, could be produced by fusion, they still would be far from resembling those we are acquainted with, which essentially contain some particles of water, as I have elsewhere shewn.

Perhaps some may say, that the same difficulties occur in accounting for the crystallization of granite in the moist way; on mature consideration, however, it will readily be seen, that the causes of coadunation in the dry and moist way are very different, and that their effects should also be different; for supposing the earths that enter into the composition of granites dissolved in the moist way, their precipitation and imperfect crystallization may be ascribed to the union they contract with each other, forming masses of each of the constituent ingredients of granite, which water can no longer hold suspended; hence the precipitation of each of the three species of stone is nearly contemporaneous, whereas if the formation of these ingredients should take place in the dry way, it would necessarily be successive keeping place with the successive diminutions of heat, and then the above-mentioned consequences would naturally ensue.

The state of the granatic ingredients in fusion, which I have above given, agrees pretty nearly with that presented by Sir James himself; he supposes the quartz felspar, shorl, mica, and garnet, &c. melted together, and the most fusible of them to be the menstruum in which the rest are dissolved, and that they differ from each other in their properties of solution, as salts differ from each other. Some of them being more soluble in the menstruum when very much heated, than when it is comparatively cold, and others may be soluble in it when little warmer than its point of congelation. "If then we say, for instance, that the congelating point of the solvent is 1000° of Fahrenheit, and if the solution is at the temperature of 2000°, we may conceive one portion of the matters dissolved, as held by the simple dissolving power of the menstruum, and another as held by means of its elevated temperature; when therefore a mass of this kind is allowed to cool very slowly, those substances held in solution by the heat of the solvent, will first separate, and being formed in a liquid, will assume their crystalline form with regularity." This consequence is truly deduced from the Baronet's hypothesis, but being contrary to fact, discovers the falshood of that hypothesis; for if any of the fore-mentioned component parts of granite can be said to be held in solution by the high heat of the solvent, it is surely the quartz; now the quartz is scarce ever found regularly crystallized when forming a component part of granite, as all mineralogists attest, and is a matter of universal observation.

"But the Baronet continues—"whereas those substances which were held by the menstruum simply as a fluid, will not separate until the congelation of the solvent itself takes place, when the crystals of the various substances will intermix and confound the regularity of form which each would have assumed if left to itself. In this manner one of the common kinds of granite will be produced, consisting of perfect crystals of shorl, mica, or garnet, inclosed in a confused mass of felspar, quartz, and shorl."—This conclusion



is as objectionable as the foregoing ; for not to mention that granites, in which shorl, and especially garnets are found, are far from being common assured by shorl and garnet, approach more to the fusibility of felspar, (the supposed menstruum) than either quartz or mica. These, therefore, are those which should crystallize without any regular form in the Baronet's hypothesis, and not the quartz and mica ; which is just the contrary of what he himself has observed, for he tells us, p. 9, " he found the crystals of felspar regularly " defined."

Sir James has since, very wisely, declined justifying his theory of the formation of granite by fusion ; and by the advice, Dr. Hope very properly applied himself to experiments on various species of *whin*, a denomination which in Scotland comprehends grunstein, basalt, trap, wacken, and porphyry.

Porphyry stones, in which, except the last, none of the component ingredients are found regularly crystalized, and on the last he has made no experiment. The former, he tells us, were softened or fused in a heat of from  $38^{\circ}$  to  $55^{\circ}$  of Wedgwood, the glasses to which they were reduced were softened on a range of from  $15^{\circ}$  to  $24^{\circ}$ , and the masses of the original stony appearance, to which these glasses, reduced by slow cooling, were softened in degrees of heat from 32 to 45. To the formation of these last he constantly applies the term *crystallization*, and calls them *crystalites*. To the vague term of crystalization I must however object, for as those stones in their original state present no regular crystals, but are at most internally, and imperfectly crystalized, so they must be when reduced from a glassy state, to one resembling their original, and thus discover rather a *nifus* towards crystalization, than perfect crystals, which latter the term crystalization generally applied, would lead us to expect.

Before I proceed to the detail of these experiments, I must observe, that the different fusibilities of these *crystalites*, as he calls them, indicate a very different state from that in which they originally existed ; the former requiring a heat of from  $32^{\circ}$  to  $45^{\circ}$ , and the latter a heat of from  $38^{\circ}$  to  $55^{\circ}$ , the reason of which is easily discovered, when the two states are deduced from a different origination, but is in vain sought for when both are to be deduced from one and the same origin.

Passing over the general preliminary accounts of these experiments, which are to be found from p. 7, to p. 10 of this dissertation, I shall now examine the most important particulars of each, as far as they give occasion to any striking observations. In this examination I am much assisted by the ingenious, accurate, and skilfully conducted analyses of Dr. Kennedy, who bids fair to rival the excellence attained by the greatest masters of that sublime and difficult art.

(To be concluded in our next.)

Experiments



## II.

*Experiments on the Combustion of the Diamond, the Formation of Steel by its Combination with Iron, and the pretended Transmission of Carbon through the Vessels. By Sir GEORGE STUART MACKENZIE, Bart.*

TO MR. NICHOLSON.

SIR,

THE account which I now send you, of some experiments with regard to the properties of the diamond, and the formation of steel, was read at a meeting of the Royal Society of Edinburgh, on the 3d of February last. By giving it a place in your valuable Journal, you will much oblige,

Sir,

Your obedient servant,

Edinb. 1st. May, 1800.

GEORGE S. MACKENZIE.

The phænomena exhibited by the diamond during combustion have been described by various authors \*, but the temperature at which this combustion takes place has not as yet been ascertained. I have made several experiments on diamonds with a view both to satisfy myself, as to their appearance while burning, and also to determine at what degree of Wedgwood's pyrometer they begin to be consumed. My experiments were conducted in the following manner †.

A diamond was placed on a thin flat piece of baked Cornish clay, and introduced into a muffle, previously heated red hot. It soon acquired the same redness as the muffle, but in a few moments more, became distinguished by a bright glow ‡. It was then removed, and on examination, its transparency and lustre appeared to be affected. It had a slight milky appearance §, its angles were blunted, and its bulk was also considerably diminished. After being replaced in the muffle, it was consumed slowly, and at last disappeared entirely.

The French chemists take notice of their having observed on diamonds after being subjected to a strong heat, black specks, and a blackishness upon their surfaces. Mitouard says, that in one of his experiments, a diamond had become as black as jet. In the many

\* D'Arcet, Rouelle, Cadet, Lavoisier, &c. Journal de Physique for 1772, 73, 89.

† All the diamonds used in these experiments were cut and polished.

‡ This glow is described by D'Arcet and Rouelle as being more or less bright, in different diamonds, exposed to heat at the same time. This difference, I suppose, was owing to their being nearer to, or farther from the mouth of the muffle, and consequently differently exposed to the action of the air.

§ "Qui est d'une transparence un peu laiteuse." Journ. de Ph. Vol. I. for 1772, p. 485.

experiments

experiments detailed in the *Journal de Physique*, this effect was not uniformly produced, and has not been properly explained. I did not observe any of these appearances on diamonds which had been exposed to various degrees of heat between  $15^{\circ}$  and  $30^{\circ}$ . There was a sort of luminous haze round the diamond while burning, but so feeble as to be scarcely perceptible\*.

During the combustion of a diamond, I placed beside it a small piece of plumbago, and observed, that the latter exhibited a luminous appearance similar to that of the former, but it began at a lower temperature. The air being excluded for a few minutes by closing the mouth of the muffle, the diamond and plumbago both lost their brightness; but this soon returned, after the air was allowed to circulate in the muffle, and was much increased by blowing on them with a pair of bellows.

I exposed a diamond fixed in a fine platina wire to the action of the blow pipe, during which treatment its consumption was extremely slow, owing, I presume, to the impurity of the air blown upon it.

In order to ascertain at what temperature this substance begins to be consumed, I placed a diamond with a pyrometer on a piece of baked clay, and pushed them gradually into the muffle. As soon as both were perfectly red throughout, the pyrometer was withdrawn, and indicated  $13^{\circ}$ . The diamond had acquired the dim milky appearance already mentioned. The diamond and pyrometer were replaced in the muffle, and the heat being slowly increased, till the glow appeared, was continued as equal as possible, till the diamond was totally consumed: the pyrometer was then measured, and indicated  $14^{\circ}$ †. In another diamond, the heat requisite to produce the glow was  $15^{\circ}$ , and at this temperature it was wholly consumed.

These observations were made in presence of Lord Webb Seymour, Sir James Hall, and Dr. Kennedy.

The experiments above-mentioned were several times repeated with different diamonds: and the result of the two last shows, that the heat required for their combustion is far below what has hitherto been supposed necessary.

The experiment of Guyton, in which the diamond was totally converted into carbonic acid by its combustion in oxygene gas, has afforded a decisive proof of its identity with carbon. This important discovery has been farther confirmed by obtaining steel from the union of diamond with soft iron. I repeated the latter experiment in the following manner.

\* "On a aperçu à leur surface, déjà diminuée de volume, une flamme légère, et entièrement semblable, pour la couleur, à celle que l'on voit onduler sur une portion de Phosphore exposé à l'air libre." *Journ. de Ph. Vol. II. for 1772, p. 114.*

† The pyrometers used in these experiments were made some years ago by the late Mr. Wedgwood; and are the same with those which he has described in Vol. 76 of the *Philosophical Transactions of London*. I mention this because those lately sold at the manufactory, do not agree with the former standard.



Into a small hollow cylinder of soft iron, closed at one end, I put some diamonds, and having introduced into the cavity above them a stopper of the same soft iron, which fitted exactly, the two pieces were rivetted together at the top. The stopper was nearly in contact with the diamonds, but I did not fill up the intermediate space with iron filings, as the French chemists did, for fear of introducing some fragments of steel from the file. Having placed the cylinder in a Hessian crucible, I surrounded it with a mixture of dry sand and clay\*. I luted a lid on this crucible, and placed it in another, on which a lid was also luted, and a small clay case containing a pyrometer piece, was attached to its outside. I wished to subject the iron to a heat not exceeding  $150^{\circ}$ ; for which purpose I placed the crucibles in a forge. After they were red hot, the fire was raised, till I thought it had attained the pitch desired, and was then continued as equal as possible for an hour. The pyrometer indicated  $151^{\circ}$ .

On opening the crucibles, I found that the upper part of the iron had been melted, and I observed several bright metallic globules adhering to the compacted mass of sand and clay next the iron. The lower part of the cylinder retained its shape, but except a portion of the bottom which remained smooth, was blistered on the surface. Having polished both ends of it, I found that on touching them with diluted nitric acid, they exhibited the spot which that acid usually produces on steel. The spot on the end which had been fused, was considerably darker than that on the other. I cut off the portion which had remained smooth, and having heated it red hot, and plunged it into cold water, it became so hard, that no impression could be made on it with a file. Several cavities were found within the cylinder, but the diamonds had totally disappeared.

The whole cylinder was thus converted into steel; one end being reduced to the state of cast steel, and the other remaining in that of cemented steel; from which it seems probable, that the upper part also had passed into the state of cemented steel before it began to melt. It may therefore be inferred, that diamond may be combined with iron so as to form steel by the simple process of cementation.

Mr. Mushet, of the Clyde Iron Works, has lately published in the Philosophical Magazine, an account of certain processes, with a view to prove "That the experiment performed at the Polytechnic School respecting the conversion of iron into steel, by means of the diamond, was not conclusive." The manner in which Mr. Mushet's experiments were conducted, appeared to me to be liable to some objections; and it was therefore necessary to ascertain, how far the diamond had contributed to the formation of the steel in the last experiment, by exposing part of the iron of which the cylinder was made to the same degree of heat in similar circumstances. Accordingly, a portion of the same iron was imbedded in sand and clay, and the apparatus was arranged, and the forge managed precisely as in the last experiment. At the end of an hour, the crucibles being withdrawn from the

\* The quartz sand, called Lynn sand, washed and heated red hot--Stourbridge clay.



fire, and allowed to cool, I found that one end of the cylinder was slightly blistered; but otherwise had retained its shape and original qualities. The temperature was  $152^{\circ}$ .

I may here once for all observe, that I took care to examine whether the iron made use of was real soft iron, before it was subjected to experiment. I likewise, in every instance, reserved a part of the iron, as a standard with which I compared that on which the experiment had been made. In this I was assisted by a skilful workman, who in my presence made trials of the hardness and malleability of the metal when heated to various degrees of temperature, and also when cold, after having been ignited and plunged into water. Whenever therefore, in future, I mention soft iron as remaining unaltered after an experiment, I wish it to be understood, that it was subjected to these proofs of its malleability, and that it had the same qualities with the portion of soft iron that had been reserved.

This result affords a proof, that iron exposed to a heat of  $150^{\circ}$  during one hour, cannot be converted into steel without the aid of carbon; and that in the first experiment, the conversion into steel was solely to be attributed to the action of the diamond.

Mr. Musket's doubt, "whether the diamond afforded even one particle of carbon to the iron," is thus clearly removed. In the paper above-mentioned, he states it as his opinion, founded on his own experiments, that when soft iron is exposed to a high temperature, carbon dissolved in caloric penetrates the crucibles, and the rest of the apparatus in which it is enclosed, and converts the iron into steel. In order to determine how far this opinion was well founded, I made the following experiments:

My first object was to expose soft iron to strong heat, in an apparatus which might absolutely exclude all carbonaceous matter, except such as might be so dissolved. This I hoped to accomplish by making use of compact crucibles, and, for greater security by the interposition of some substance between the iron and the crucibles.

On considering with Dr. Kennedy what substance would most effectually answer this purpose, we agreed that pure white felspar (adularia) would be the best; for Dr. Kennedy, in the course of an analysis of this substance, having found that it began to pass into the state of glass about  $90^{\circ}$ , we concluded, that before the heat was of sufficient intensity to melt the iron, it would be surrounded by a vitreous mass perfectly free from carbonaceous matter; and, as we presumed, impervious to the action of carbon from without. Another reason for using adularia was, that its action on crucibles is but slight, which would prevent any risk of the experiment failing by their being melted.

For the processes next to be described, I made some small crucibles of the porcelain clay of Cornwall, and baked them in a strong heat. Into one of these I put some felspar in fine powder, upon which having placed a small cylinder of soft iron, the crucible was filled with the felspar, so that the iron was completely surrounded with it. On this crucible I luted a lid of the same clay, and placed it in a Hessian crucible, which was filled up with powdered felspar. Having also luted a lid on the Hessian crucible, I attached to its outside a small clay case containing a pyrometer piece, and covered the whole with a coating of Stourbridge clay and quartzysand. The crucible and its contents were then placed  
in

in an air furnace, and the heat was raised gradually for an hour. During a second hour it was continued at its highest pitch; after which the apparatus was removed from the fire\*. The heat marked by the pyrometer was  $152^{\circ}$ .

The iron had retained its shape perfectly, and its properties remained unchanged. The felfpar was reduced to a glafs, perfectly transparent and colourless, except that it had received a greenish tinge where it was in contact with the iron. This, I presume, it derived from a small portion of oxide formed on the surface of the iron, by the air unavoidably included in the crucibles.

Wishing to exclude the felfpar from the iron, I varied the process by placing a piece of iron alone in a cornish clay crucible, luting on a lid, and imbedding this in another crucible filled with felfpar. The heat was continued for an hour and a half. I found that the melted felfpar had penetrated into the interior crucible, and that part of it had reached the iron which still remained unaltered.

In order effectually to prevent the felfpar from coming into contact with the metal, I next put a piece of iron into one of the small cornish clay crucibles, and inverted another of a larger size over it. The two crucibles were luted together with a mixture of Cornish clay baked in a heat of  $160^{\circ}$ , and a little of the same clay in its raw state. I placed them in a Hessian crucible, surrounded them with fragments of felfpar, and luted on a lid as before. The heat was managed as in the former experiment, and continued in its greatest intensity for an hour and a half. I found the small crucibles intire, and imbedded in the glafs of the felfpar. The oxide of the iron had acted on the interior crucible, and formed a small quantity of brownish glafs. The iron retained its shape and softness, and possessed none of the properties of steel.

Mr. Mushet is of opinion, that carbon dissolved in caloric, penetrates but slowly through earthy bodies. It therefore occurred, that he might object to these results as unsatisfactory, because I had not allowed sufficient time for the carbon to find its way through the thick mafs of crucibles and felfpar in which the iron was imbedded. This I hoped to obviate by the following experiments.

Having placed in a Hessian crucible a piece of soft iron, weighing about half a pound troy, I surrounded it with sand. This crucible was included in two others of the same kind, and the space between each being filled with sand, lids were luted on, and the whole was placed in the reverberatory at Mr. Barker's Iron Foundry, and allowed to remain in its utmost heat for six hours. When taken out, I found that the ashes carried up by the flame had vitrified a great part of the crucibles, so as to form two small apertures in the interior crucible which contained the iron; but to my surprize I found the iron unaltered, except that it was slightly oxidated on the surface. The pyrometer was destroyed.

In a second experiment with another piece of iron, I filled the interstices between the crucibles with lute made of sand and clay. The heat was continued for four hours only,

\* In all these experiments the fuel was coak.



at the end of which I found the outer crucible vitrified, but the interior ones intire. The pyrometer measured  $153^{\circ}$ , and the iron remained unchanged.

Finding that under the circumstances above-mentioned no carbon penetrated to the iron, I now resolved to repeat Mr. Mushet's first experiment, following exactly the process which he has described; and as he does not inform us what kind of crucibles he used, I took English crucibles, which are less compact than the Hessian.

Mr. Mushet says, that he used sand, obtained by pounding the stone of which the furnace hearths at Clyde Iron Works are made. I am unacquainted with this particular stone; but it is well known, that many sand-stones contain coaly matter. I therefore made use of Lynn sand.

Into an English crucible I put three small cylinders of soft iron, and on this luted a lid made of sand, with a very small proportion of clay. I placed this in a larger crucible containing some sand, and covered it up with the sand, till the outer crucible was almost full. Having adjusted a lid to it, I placed the whole in the air furnace. The fire was managed as in the former experiments, and the greatest heat continued for one hour and a half, when the crucibles were taken out entire.

Part of the interior crucible, and the oxide of the iron, occasioned by the included air had entered into fusion, and formed a small quantity of brownish glass. The three pieces of iron adhered slightly together, and on those parts which had not been in contact with the glass, there was a thin film of oxide, which easily scaled off: but the shape of the cylinders was not altered. On separating them, I found that they retained all the qualities of soft iron. Their softness must therefore have been such as to unite them by a process similar to that of welding.

As Mr. Mushet informs us, that in one of his experiments his crucible was cracked, I conceived that carbon might have found access to the iron through the aperture in the form of carbonic acid, of flame, or of smoke. In order, therefore, to ascertain how far the process was affected by this circumstance, I followed the detail of his experiment precisely; using a cracked crucible, and sand pounded from a sand stone, in which, however, I could discover no traces of coaly matter. The heat in this experiment was continued for an hour and a half. The iron remained unchanged.

With the same view I made a second experiment, which I imagined would be still more decisive. A small shallow crucible containing a cylinder of soft iron, similar to those formerly used, was placed in a larger one; through the sides of which I had bored three holes. In this instance, no sand was introduced into either of them. Having adjusted a lid to the outer crucible, I exposed the whole to the greatest heat that the forge could produce for forty-five minutes; after which the iron was found not to have received any alteration in its qualities.

In the experiments already described, the heat generally exceeded  $150$ , but was not of sufficient intensity to melt the iron, and the results would scarcely have satisfied me, had I

not



not found that soft iron, in one solid piece, in fragments, or in filings, may be perfectly fused without undergoing any change in its properties.

A cylindrical piece of soft iron, about an inch in length, and half an inch in diameter, after being placed on its end in a Hessian crucible, and surrounded with quartz sand, was exposed to a strong heat in a furnace. The upper half of the cylinder lost its shape and sunk down: but the iron retained its original properties.

The fusibility of soft iron is proved in a more satisfactory manner by the following experiments: five thin flat pieces of soft iron, making when laid together half a cubic inch, softened into one solid mass, which still possessed all the properties of malleable iron.

I took two Cornish clay crucibles, and in one placed a small cylindrical piece of soft iron, which weighed only twenty-five grains, and in the other the same weight of filings of soft iron. Both were placed, together with a pyrometer, in a Hessian crucible, on which a lid was luted, and the apparatus was exposed to the heat of the forge for three quarters of an hour. Both the cylinder and the filings were melted into buttons. The heat was  $158^{\circ}$ .

Wishing to ascertain whether oxide of iron would unite with soft metallic iron when in fusion, and render it brittle, I mixed some iron filings with a tenth part, by weight, of scales of iron from a blacksmith's anvil, in the state of fine powder, and put the mixture into a Hessian crucible, included within a larger, which I placed in the forge, together with another containing some of the same filings without oxide, and a pyrometer. The heat was applied for three quarters of an hour, after which I found a solid button in each of the crucibles. The fusion was so complete, that in the crucible containing the mixture of filings and oxide, some of the melted iron had penetrated through a very small hole, and remained between the interior crucible and the exterior. The heat indicated by the pyrometer was the same as in the last experiment,  $158^{\circ}$ . The workman with whom I tried the malleability of these two buttons, said, he could discover no difference between them, and that he thought they consisted of the softest and most ductile iron he had ever examined.

As the iron was here found to be melted at  $158^{\circ}$ , and as in preceding experiments in the furnace, and also in Mr. Barker's reverberatory, pieces of iron did not melt at  $153^{\circ}$ , it appears that the heat requisite for reducing soft ductile iron to fusion, exceeds  $153^{\circ}$ , but is not higher than  $158^{\circ}$ .

From the result of the foregoing experiments, I hope it is evident, that soft iron cannot be converted into steel, by carbon penetrating from the fuel through the crucibles. As Mr. Musket has not proved by humid analysis, that his results contained carbon, I am inclined to think, that in certain circumstances, iron may perhaps be combined with earths, so as to form a compound in some degree resembling steel. The experiments published by Clouet seem to afford grounds for this supposition. One of these I have repeated, namely, the fusion of iron filings with a mixture of carbonate of lime, and powdered crucibles, and obtained a button which became very hard, when heated red hot and plunged

plunged into water. But I did not succeed when I employed pieces of nails instead of filings; for the nails remained unmelted and soft. I propose to make some experiments on the fusion of iron with earths, and to analyse the products, in order to learn whether such a combination can take place,

I shall conclude by relating a new experiment, to which I have subjected the diamond, by which the identity of this substance with carbon is still farther confirmed.

Having prepared some pure oxide of iron from a solution of the sulphate, by precipitation with caustic ammonia, I mixed a small quantity of it with one fourth of its weight of diamond powder, prepared in the following manner:

The diamond being reduced to powder in a steel mortar, was boiled in muriatic acid, to dissolve the iron which might have been abraded from it. After proper edulcoration, it was heated in a muffle to burn off the carbon of the steel which remained after treatment with the acid, and which rendered the powder of a grey colour. I observed the coaly matter take fire at the edge of the heap of powder next the strongest heat, and gradually spread itself, till at last the whole appeared as if burning. The glow through the powder ceased soon after, and on removing it I found it perfectly clean and white. From the diminution of the original weight of the diamond, I found that a part of it had also been consumed.

The mixture of oxide and diamond powder was put into a Cornish clay crucible, and exposed to a pretty strong heat for half an hour, after which the oxide was found to be reduced into a metallic button of cast iron.

Another portion of the oxide of iron used in this experiment, was not reduced when placed in the same circumstances without the diamond.

### III.

*Account of certain Phenomena observed in the Air Vault of the Furnaces of the Devon Iron Works\**; together with some practical Remarks on the Management of Blast Furnaces: By Mr. JOHN ROEBUCK, in a Letter to Sir JAMES HALL, Bart †.

SIR,

I HAVE examined my memorandums, concerning the observations I made on the condensed air in the *air vault* of the Devon iron works, near Alloa; and, according to your request, I now transmit you an account of them; and also of an experiment I made, when

\* These iron works are on the banks of the river Devon, which runs into the Frith of Forth near Alloa. They are 3 miles from Alloa, and 2 from Stirling.

† Communicated by him to the Royal Society of Edinburgh, and inserted in the fifth Vol. of their Transactions.

It was omitted to be mentioned, that Sir James Hall's paper in our two last Numbers is inserted in the same volume.



a partner and manager of these works, in order to increase the produce of blast furnaces.

The two blast furnaces at Devon are of large dimensions, each being 44 feet high, and about 13 feet wide in the boshes, or widest part, and are formed on a steep bank, by two pits sunk in a very solid stratum of coarse grained freestone.

These pits were afterwards shaped and lined in the usual manner of blast furnaces, with common bricks and fire bricks, and the hearth was laid with large blocks of the stone that had been dug out, and which serve the purpose of fire stones. At the back of the two furnaces, next the bank, the air vault is excavated, and formed by a mine drove in the solid rock, distant from the furnaces about 16 feet. The bottom of the air vault is only about 4 feet higher than the level of the bottom of the furnaces. This vault has an aperture at one end to receive the air from the blowing machine, and has two at the opposite end, one of which receives the eduction pipe, and the other is a door to give admittance occasionally into the vault. As the rock is extremely close and solid, the vault is dry, except that a little water oozes very gently from the side next the bank in small drops; and does not appear to exceed an English pint in 24 hours.

These furnaces are provided with air, or blast, as it is termed, by the means of a fire-engine of the old, or Newcomen's construction. The diameter of the steam cylinder is  $48\frac{1}{4}$  inches; and the square area of its piston being about  $1866\frac{1}{2}$  square inches, the power of this sort of engine cannot be rated at more than 7 lb. to the square inch, amounting in all to about 13062 lb. This power was employed to work an air pump, or blowing cylinder, of 78 inches diameter, and about 7 feet long. The number of square inches on the piston of the air pump is 4778, and therefore this area, being multiplied by  $2\frac{3}{4}$ , will produce 13139, being a resistance that nearly balances the above-rated power, and shows that the air, which was expelled from the air pump, could not be condensed more in the ordinary way of working, than with a compressing power of about  $2\frac{3}{4}$  lb. on each square inch. As the engine was not regulated, at first, to make a longer stroke than about 4 feet 8 inches, only one furnace being used, the quantity of air expelled at each stroke of the machine was about 155 cubic feet, which it discharged through a valve into the air vault, about 16 times in a minute. When two furnaces afterwards were blown, the engine was regulated to work much quicker, and with a longer stroke. The air vault is 72 feet long, 14 feet wide, and 13 feet high; and contains upwards of 13,000 cubic feet, or above 80 times the contents of the air pump. The top, sides, and bottom of this vault, where the least fissure could be discovered in the beds of the rock, were carefully caulked with oakum, and afterwards plastered, and then covered with pitch and paper. The intention of blowing into the vault is to equalize the blast, or render it uniform, which it effects more completely than any machinery ever yet contrived for the same purpose. The air is conducted from the vault by the eduction pipe, of 16 inches diameter, into an iron box or wind chest, and from this it goes off to each furnace, in two smaller pipes that terminate in nozles, or blow-pipes, of only  $2\frac{1}{2}$  to  $3\frac{1}{4}$  inch diameter, at the twee of the surface.



When the surface was put in blast, after having been filled with coakes, and gently heated for more than six weeks, the keepers allowed it to have but little blast at first, giving it a small blow-pipe of about  $2\frac{1}{4}$  inch diameter, and likewise letting off a very considerable quantity of air, at the escape, or safety valve on the top of the iron wind chest, as it is a received though erroneous opinion among them, that the blast must be let on very gradually for several months. From the construction of this valve, it was impossible to ascertain the exact proportion of the blast they thus parted with, but I believe it was very considerable. The consequence was, that the furnace, after it had been in blast for several days, never seemed to arrive at its proper degree of heat, but was always black and cold about the tweer in the hearth, and appeared in danger of choking, or gobbing, as it is termed.

After various experiments tried in vain, by the keepers and the company's engineer, and others, (indeed they tried every thing, except giving the furnace a greater quantity of air, which, as I afterwards ascertained, was all that it wanted), they concluded, that the air vault was the cause of the whole mischief; and, to confirm their opinion, they said they had now discovered that water was, in considerable quantities, driven out of the air vault through the blow-pipe, which cooled the furnace; and they insisted, that the power of the engine was such as to force water out of the solid rock; so that this method of equalizing the blast never would succeed. The other managing partner was so much alarmed by these representations, that he began to consult with the engineer, and others, about finding a substitute for the air vault at any expence.

As the plan of the blowing apparatus had been adopted at my recommendation, and was now so loudly condemned on account of the water, I had other motives, than mere interest, for trying to become better acquainted with the phænomena attending it. I accordingly determined to go into the air vault, and to remain inclosed in the condensed air while the engine was blowing the furnace. It is an experiment that perhaps never was made before, as there never existed such an opportunity. I could not persuade the engineer, or any other of the operative people about the work, to be my companions, as they imagined that there was much danger in the experiment. Mr. Neil Ryrie, however, one of the clerks of the Devon company, had sufficient confidence in my representations to venture himself along with me.

The machine had been stopped about two hours previous to our entering the vault, and we found a dampness and mistiness in it, which disappeared soon after the door was shut fast upon us, and the engine began to work in its usual manner. After four or five strokes of the engine, we both experienced a singular sensation in our ears, as if they were stopped by the fingers, which continued as long as we remained in the condensed air. Our breathing was not in the least affected. I had no thermometer with me, but the temperature of the air felt to us the same as that without the vault. Sound was much magnified, as we perceived, when we talked to each other, or struck any thing; particularly, the noise of the air escaping at the blow-pipe, or waste valve, was very loud, and seemed

to return back to us. There was no appearance of wind to disturb the flame of our candles; on the contrary, I was surprised to find, that when we put one of them into the eduction pipe, which conveys the wind from the vault to the furnaces, it was not blown out. There was not the smallest appearance of any drops of water issuing out of this pipe. The ouzing and dropping of water from the side of the rock, next the bank, seemed the same as before the condensation was made in the vault. In short, every thing appeared, in other respects, the same as when we were in the common atmosphere. Having remained about an hour in the condensed air, and satisfied ourselves that no water, during that time, that we could in the least discover, was agitated and forced out of the rock and vault by the power of the blast, as was imagined and insisted on, we gave the signal to stop the engine. As soon as it ceased to work, and the condensation abated, and before the door of the vault was unscrewed, *the whole vault, in a few seconds, became filled with a thick vapour, so that we could hardly see the candles at four or five yards distance.* The door being now opened, the work people, anxious to know our situation, and what had occurred, came into the vault, and prevented any further observations.

I now endeavoured to account for this curious appearance of the water, which only shewed itself occasionally, in very small quantities, at the tweer, at a hole I ordered to be made in the bottom of the wind chest to collect it more accurately, for it never was observed, but either when the engine, after working slowly, was made to work quicker, or, after having been stopped for a few minutes, was set to work again.

I considered the vapour which we had discovered in the vault to arise from the moisture of the side of the rock next the furnace, which being expelled by the great heat of the furnace, and converted into vapour, was able to force its way through the pores of the rock into the vault, but that being in a manner confined within the rock, by the pressure of the condensed air, it found itself at liberty to come into the vault, only when the condensation abated considerably, or was totally removed by the going slow, or stopping of the engine. It also occurred to me, that the air, in a state of condensation, might possibly be capable of holding a greater quantity of water in solution, which might precipitate suddenly into vapour or mist when the condensation abated. I imagined, therefore, that the very small quantities of water we at times discovered, proceeded from nothing else but this vapour, in its passage to the furnace along with the blast, being condensed into water, by the coolness of the eduction pipe and iron wind chest. The quantity of water did not appear to amount to a gallon in twenty-four hours.

A few days after I had made this experiment, the water ceased entirely to make its appearance, either at the tweer, or at the hole in the wind chest; but the furnace did not come into heat for a long while after, and indeed not till the keepers let much more air into it by a larger blow-pipe, and allowed less air to escape at the safety valve. It is probable that the rock was now become perfectly dry by the continued heat of the furnace.

My experiment had the good effect to remove all the prejudices against the plan I had adopted of blowing the furnaces, and likewise prevented the other partner from laying out



a large sum of money, by stopping the works, and altering the blowing machinery. Indeed, it has since been admitted, by all who have seen it at work, to be the most simple and effective method of equalizing the blast of any yet put in practice.

This experiment led me, some time afterwards, to apply a wind guage that I contrived, to ascertain precisely the state of the condensation of the air thrown into the furnaces. I found that a column of quick-silver was raised five inches, and sometimes, though seldom, six inches, and, in the interval of the engine to receive air into the air pump, it fell only half of an inch. At this time only one furnace was worked. But when two furnaces were in blast, the engine only raised the mercurial guage about 4 inches, because the Devon company, for certain reasons, did not, while I continued a partner, think proper to allow the blowing machinery to be completed, by the putting to work their second boiler of 20 feet diameter for the fire engine, according to my original design, which, by adjusting the machinery, would have enabled us to blow two furnaces, with two boilers, with as much effect, in proportion, as one furnace with one boiler. This instrument had the advantage of enabling the work people to discover the real power of their blast, and know the exact condition of the air valves, and the gearing of the blowing piston; for if these were not tight, and in order, (although the engine might, to appearance, be doing well, by making the same number of discharges of the air pump as usual *per minute*), yet the wind guage would not rise so high, and would shew that there was an imperfection somewhere, by reason of a quantity of air escaping at the valves, or piston, that could not so easily otherwise be known. This contrivance was considered as of much use, and was afterwards always quoted in the company's journal books, to shew the actual state of the blowing machine, in comparing the daily produce of the furnaces.

I hope you will not think me tedious, when I explain to you another experiment, which appears to me to be of considerable importance to all manufacturers of cast iron.

I had reason to conjecture, from my own observations on the effects of blowing machinery on blast furnaces, as well as from the knowledge I had acquired from my father Dr. Roebuck, and from my communications with other experienced iron masters, that a great part of the power of such machinery was misapplied in general practice, by throwing air into furnaces with much greater velocity than necessary, and that, if this velocity was, to a certain degree diminished, the same power, by properly adjusting the blowing machinery, of whatever nature, would be capable of throwing into the furnace a proportionally greater quantity of air. For, "*Since the quantities of any fluid, issuing through the same aperture, are as the square roots of the pressure;*" it follows, that it would require *four* times the pressure, or power, to expel *double* the quantity of air, through the same aperture, in the same time: but if the area of the aperture was doubled, then the quantity of air expelled by the same power, and in the same time, would be increased in the ratio of the square root of 2 to 1, though its velocity would be diminished exactly in the same proportion. Again: I considered that the quantity and intensity of heat, produced in blast furnaces, and consequently its effects in increasing the produce, might be



only in proportion to the quantity of air decomposed in the process of combustion, without regard to its greater velocity; that is to say, whether or not the same quantity of air was forced, in the same time, into the furnace through a small pipe, or through one of larger dimensions; for, in attending to the process of a common air furnace for remelting of iron, where there is a very large quantity of air admitted through the large areas between the bars, it is well known, that a much greater intensity of heat is produced than takes place in a blast furnace, and yet the air does not enter into the fire through the bars with increased density or great velocity. I therefore thought it probable, that increasing the *quantity* of air, thrown into the blast furnace in a considerable degree, although the *velocity or density* might be much less, would have the effect of increasing its heat, and operations, and produce. And as, from the principles above stated, with regard to the machinery, I saw I could greatly increase the quantity of air thrown into the furnace, by enlarging the diameter of the blow-pipe, and regulating the engine accordingly, without being obliged to employ more power, I was anxious to make this experiment.

A system of management, of which I did by no means approve, was adopted by the other partners of the Devon company, soon after the works were begun to be erected; and, in the prosecution of it, they ordered their second furnace to be put in blast, without permitting those measures to be taken that were necessary to provide and maintain a sufficient stock of materials; and also without allowing their blowing machine to be completed, according to the original design, by the addition of its second boiler. As might have been expected, a trial of several months to carry on two furnaces, with only half the power of steam that was necessary, and an inadequate stock of materials, proving unsuccessful, the company, as a remedy, instead of making up the above deficiencies, ordered one of the furnaces to be blown out, and stopped altogether. This improper measure, however, afforded me the opportunity of immediately putting in practice the plan I have mentioned.

When one of the furnaces was stopped, the other continued to be blown by a blow-pipe of  $2\frac{3}{4}$  inches diameter, and the produce of the furnace, for several weeks thereafter, was not 20 tons of iron *per week* at an average. The engine at this time was making about 16 strokes a minute, with a stroke of the air pump, about 4 feet 8 inches long; but when I altered the diameter of the blow-pipe, first to 3, and immediately after to  $3\frac{1}{4}$  inches diameter, and regulated the working gears of the engine, so as to make a stroke of 5 feet 2 inches long, and about 19 strokes in a minute, on an average, the produce was immediately increased. It continued to be, on an average of nine months immediately after this improvement, at the rate of 33 tons of iron *per week*, of as good quality as formerly; for during this period, from the 21st November 1795 to July 30, 1796, this one furnace yielded 1188 tons of iron. No more coals were consumed in working the blast engine, or other expences about the blowing machine incurred, and therefore no more power was employed to produce this great effect. It is also of much importance to remark, that the

consumption of materials, from which this large produce was obtained, was by no means so great as formerly. The furnace required very considerably *less fuel, less ironstone, and less limestone*, than were employed to produce the same quantity of iron by the former method of blowing; and according to the statements made out by the company's orders, as great a change was effected in the economical part of the business.

From the success of this experiment, so well authenticated, and continued for several months, I am led to be of opinion, that all blast furnaces, by a proper adjustment of such machinery as they are provided with, might greatly and advantageously increase their produce, by assuming this as a principle, viz. "*That with the giving power it is rather by a great quantity of air thrown into the furnace, with a moderate velocity, than by a less quantity thrown in with a greater velocity, that the greatest benefit is derived, in the smelting of iron-stones, in order to produce pig-iron.*" However, it is by experiment alone, perhaps, that we can be enabled to find out the exact relations of power, velocity, and quantity of air requisite to produce a *maximum* of effect\*.

But, an unfortunate disagreement among the partners of the Devon company, put it out of my power to make further progress in this matter, by laying me under the necessity, two years ago, of withdrawing myself entirely from the concern.

I have the honour to be, respectfully,

SIR,

Your most obedient servant,

Edinburgh, June 30, 1798.

JOHN ROEBUCK.

To Sir James Hall, Bart.

\* If  $Q$  be the quantity of a fluid, issuing in a given time through an aperture of the diameter  $D$ ,  $V$  its velocity, and  $P$  the power by which it is forced through the aperture: then the area of that aperture being as  $D^2$ , the quantity of the fluid issuing in the given time will be as  $VD^2$ , or  $VD^2 = Q$ .

Again, this quantity multiplied into its velocity, will be as the *momentum* of the fluid expelled, or as the power by which it is expelled, that is,  $V^2 D^2 = P$ , or  $VD = \sqrt{P}$ .

Here, therefore, if  $D$  is given,  $V$  is as  $\sqrt{P}$ , as Mr. Roebuck affirms. Also, because  $V = \frac{Q}{D^2}$ , and also  $V = \frac{\sqrt{P}}{D}$ ,  $Q = D\sqrt{P}$ , so that, while  $P$  remains the same,  $Q$  will increase as  $D$  increases, and  $V$  will diminish in the same ratio.

The problem, therefore, of throwing the greatest quantity of air into the furnace, with a given power, strictly speaking, has no *maximum*, but the largest aperture of which the engine can admit must be the best. It is probable, however, that there is a certain velocity with which the air ought to enter into the furnace; this will produce a limitation of the problem, which, as Mr. Roebuck suggests, is not likely to be discovered but by experiment. J. P.

In



In order to illustrate what is said above, a ground plan of the air vault and furnaces of the Devon Iron Works is given in Plate V. ; of which the explanation follows :

*Explanation of Fig. 1. Plate V.*

- A The air vault, formed by a mine drove in the solid rock of coarse grained freestone.
- B The blowing cylinder.
- C The pipe that conveys the air from the blowing cylinder to the air vault.
- D The eduction pipe that carries the air from the air vault to the iron wind chest.
- E The iron wind chest, (about  $2\frac{1}{2}$  feet cube), in which is inserted a wind-gauge, represented in fig. 3.
- FF The two blow pipes for each furnace, which terminate in apertures of  $3\frac{1}{4}$  inches diameter at the tweers of the furnaces.
- GG The two blast furnaces, placed in two pits sunk in the solid rock.
- HH The tips of the furnaces from whence the cast-iron is run off into the casting room, L L.
- O The door to give occasional admittance into the air vault.
- M The excavation, in which is placed the blowing machine.

*Explanation of Fig. 2.*

- A The end of the wind-gauge, (about 12 inches long), which is open to the atmosphere; being half filled with quicksilver.
- B The end that is inserted in the iron wind chest, and exposed to the pressure of the condensed air of the air vault.

#### IV.

*Apparatus for making the Hydraulic Engine at Schemnitz work itself without Attendance. By Mr. JOHN WHITLEY BOSWELL.*

To. Mr. NICHOLSON.

SIR,

May 7, 1800.

HAVING last summer met with a description of an engine, similar in its principles to the hydraulic machine of Schemnitz, (of which there is an account in the Number of your Journal \*, in which you did me the honour to insert my communication of a new ventilator); I was induced to contrive a method for making it work itself, as well from the utility of such an invention, as from its being obvious at first sight that it was very practicable.

\* Vol. IV. p. 13.

From.



From the interest you seem to take in the above-mentioned engine, I conclude it will not be unacceptable to you to have a description of this method of saving the labour at present employed in working it, and therefore take the liberty to send you a drawing, in which all the vessels are supposed to be open at one side, that the disposition of the pipes within may be more clearly seen.

Simplicity of form, and facility of execution, have been chiefly aimed at in this construction; and I have no doubt but it must perfectly answer the intention, or I would not trouble you with it. If, however, you should be of a different opinion, it will give me pleasure to be set right by you.

DESCRIPTION: Plate VI.

- A. The reservoir, or upper level of water.
- B. A chamber made of sufficient strength to bear the internal pressure of a column of water of the height of A above it, multiplied by its own base.
- C. A chamber of the same strength as B, but of a smaller size; it is placed at the bottom of the pit from which the water is to be raised, and under the level of the water. These chambers would be stronger with the same materials, if of a globular or cylindrical form; but the square shape is used in the drawing, merely for the facility of representing the position of the parts.
- D. A pipe, from the reservoir A which passes through the top of B, and ends near its bottom, to convey water from A to B.
- E. A pipe from the top of B to the top of C, to convey air from B to C.
- F. A pipe from the bottom of C to the level of the ground at the top of the pit, to carry off the water from the pit.
- G. A pipe from the bottom of B to carry off the water from it.
- H. A vessel to contain the water used in working the cocks; it is only placed on the top of B to save the construction of a stand on purpose for it.
- I. A cock, or moveable valve, (worked by the lever there represented,) in the large pipe D.
- K. A stop cock in the small pipe which conveys water from D to H. Its use is to make the engine work faster or slower, by letting water more or less quick into H; or to stop it altogether from working when required.
- L. A moveable valve, or cock in the small pipe L K. The lever which works it is connected by a strong wire with the lever which works I, and is balanced by a weight at its opposite extremity, sufficient to open both those cocks and shut N, when not prevented by a counter weight.
- N. A cock in the pipe G to open and shut it as wanted.
- O. A self moving valve in the pipe F, which permits the water to pass upwards, but prevents its return.
- P. A self-moving valve at the bottom of C, which permits the water to pass into C, but prevents any from passing out of it; it is furnished with a grating to prevent dirt getting in.

R, A

- R. A vessel suspended from the levers of I and L, capable of containing a weight of water sufficient to shut them.
- S. A vessel suspended from the lever of N, it must contain water enough by its weight to open N: it is connected by a chain to R, to keep it down as long as N is open.
- T. A syphon passing from the bottom of H, near its upper edge, and down again to the mouth of R.
- V. A self moving valve, of a sufficient levity to rise, when the water in B comes up to it, and close the pipe E; into which no water would else pass from B. A ball-cock, such as used in common water cisterns, would also do here.
- X. A syphon from the bottom of R rising within an inch of its top, and passing down again to the mouth of S.
- Y. A small pipe at the bottom of S; this may have a stop-cock to regulate it, which when stopped will also stop the engine.

## EXPLANATION.

The mode of this engine's working is as follows: suppose the vessels V H R and S empty of water, and the cocks K and Y open, and the vessel C full of water. The weight on the lever of L will then open the cocks L and I, on which the water from A will flow into B and H. As the water rises in B it will force the air through E into C, which strongly pressing on the water in C will force it up through the pipe F, till the water in B rises to the level of V and closes it, at which time H will be full of water, (the quantity flowing in being so regulated by the cock K) and the water will flow from it through the syphon T into the vessel R, which as it fills shuts the cocks I and L, and prevents any more water coming into B and H. When R is full, the water flows through its syphon X, which fills S, and by it opens N, which empties B of water, and keeps N open as long as there is any water in H.

When H is empty, B will be so too (being so regulated by the cock K) on which in a moment or two R and S will also be empty; which will cause the cocks I and L to open, and all things be again in the state first supposed, for a repetition of the operations described.

To stop the engine the cocks at K and Y should be shut, while S is full of water. To set it working they should be opened, and this is all the attendance it will require. As no one but an engineer should attempt to construct such an engine as this, it was useless to represent the manner of connecting the pipes by flaches, or otherwise, or the proper methods of fastening and closing all parts, which are all well known to such as have made this art their study. If this method of effecting the purpose shall be esteemed worthy of being adopted by any of them, it will give me great pleasure to have contributed to the improvement of an engine so useful, where it can be adopted.

Your very humble Servant,

JOHN WHITLEY BOSWELL.

P. S.



P. S. I beg leave to request that an error of the press in the paper on Ventilators, may be corrected, page 7, line 16 and 17, for "and this way of using it might be made" read "and for this way of using it, it might be made."

I also will thank you to notice, that my ventilator was constructed in March 1798, before Venturi's book came to England, which I understand was in November 1798. Mr. Bonnycastle, of Woolwich, to whom I shewed it, and its mode of action at that time, can no doubt recollect this circumstance.

J. W. B.

## V.

*On the Chemical Action of the different Metals upon each other at the common Temperature of the Atmosphere, and upon the Explanation of certain Galvanic Phenomena. By M. FABBRONI\*.*

THE phenomenon spoken of by Sultzcr, in his *Theorie des Plaisirs*, published in 1767, has been classed among galvanic phenomena, that is to say, the mysterious sensation which is manifested on the tongue by the contact of two metals in mutual contact, which would have excited none if they had been separately applied to that organ. In fact, I have been persuaded, that this very principle which produces an unexpected taste in this case, may also produce a convulsive contraction in the animal fibre, when it comes to touch the irritable and sensible parts uncovered at the same time. But so far from attributing these effects with all the world to an agent almost unknown, such as the electric fire, I immediately supposed them to depend upon a mere chemical operation, in the same manner as probably the sense of taste itself is produced, which renders the mechanism much more intelligible. I made reflections, and instituted experiments on this curious subject, of which I gave an account to the Academy of Florence in 1792. The volume has not yet been printed; but I think the subject is spoken of by Brugnatelli in his Journal. I have neither his abridgment, nor my own memoir at hand; I shall therefore repeat in this place nothing but what remains clearly in my recollection.

I had before observed on many occasions, that running mercury retains its metallic splendor a long time while alone and uncombined; but that its amalgam with any other metal whatever is soon tarnished or oxidized, and increases in weight in proportion to that progressive change.

I have kept, for many years, fine tin without any alteration in its argentine appearance, but this was not the case with regard to the different alloys I had made with the same metal for economical speculations.

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I had seen in the museum at Cortona, Etruscan inscriptions engraved upon pure lead, which are still in perfect preservation, though of the most remote antiquity; and on the contrary, I had found with surprize in the gallery of Florence, medals of lead of the different pontiffs, in which tin, and perhaps arsenic had been mixed, to render them more beautiful and solid, were intirely reduced to a white powder, or changed into oxide, notwithstanding they had been wrapped in paper, and kept in drawers.

I had likewise observed, that the alloy made use of in foldering the plates of copper which cover the moveable roof of the Observatory at Florence, was speedily changed, and converted manifestly into a white oxide at its extreme contact with that metal.

Lastly, I had learned in England, that the iron nails formerly used in attaching the copper to the bottoms of ships, corroded this last metal so much by their contact, that the holes soon became dilated, so as to exceed the size of the head of the nail itself.

These facts appeared to me to be amply sufficient to shew, that the metals exercise a mutual action in this case, and that the cause of the phenomena produced by their union, or contact, was to be attributed to this action.

It is known, that the metals are in general susceptible of combining together by mutual solution. We may therefore form a notion, that their tendency to mutual combination begins as soon as the particles are brought into contact. It is only by virtue of the immense superiority of their force of cohesion, that they are prevented from mutual penetration and solution in the cold. Fire is necessary to disunite and give mobility to their particles. We see this happen in amalgams which are formed without heat; and it is known, that in the manufacture of tinned plates, the tin penetrates the iron without this last metal being liquified. It is, probably, the same force of cohesion which sometimes prevents the oxidable metals from attracting oxygen with speed. If a rapid movement tends to disaggregate the particles of mercury in the midst of water, nothing more is required to enable it to assume the principle of oxidation in a very short time, by attracting it from the fluid. These facts, as well as many others of the same nature, no less common than well known, ought to have proved to philosophers, that the metals by exercising their mutual attractive force, must by the same energy diminish their respective powers of aggregation; that though neither of them separately may be able separately to attract oxygen from the atmosphere, or from water, they may acquire that power by simple mechanical touch, as they pass to new combinations. We might therefore suspect, that some, at least, of the effects produced on the bodies of animals, by the application of metallic coatings to the nerves and the muscles, may be attributed to a chemical operation; to the transition of oxygen into a combination; to the formation of a new compound; or to the developement of a soluble or sapid taste, which is so perceptibly manifested on the organ of that sensation.

Galvani, Aldini, Volta, and other philosophers equally skilful, who have so successfully directed their attention to this kind of research, not reflecting that the chemical action exerts itself with the swiftness of lightning, and, surprized at the suddenness with which the two different metals exert their effects on the animal fibre, were of opinion, that it

could only be attributed to the electric fluid. The transmission of galvanism to a distance, and by a circuit, favored their notion, which was generally received, notwithstanding the very strong objections which might, in some cases at least, be opposed to their system. In fact, some signs of electricity have been observed on the separation of two metals, which had been before placed in contact; but it is very well known, that even several chemical operations are constantly attended with a change of equilibrium in the electric fluid, and consequently by perceptible marks of electricity. Thus it is that flashes of lightning are seen in the great volcanic fusions or eruptions; and this also is one of the cases in which philosophers have taken that for the cause of these fires, which is merely one of their effects. The signs of electricity may be produced by melting a small quantity of sulphur, or of chocolate, or even by boiling or evaporating water; but assuredly these are fusions, and are not caused by the electricity. I do not pretend to exclude all electrical influence from the prodigious effects of galvanism. I propose merely to prove, that this principle is not concerned in the phenomena of Sultz, and that various other similar facts are derived from the same source.

The metals having a mutual affinity, their particles must mutually attract each other when they are brought into contact. We cannot estimate the magnitude of this force; but I think it is sufficient to weaken the force of their aggregation, so as to dispose them to engage in new combinations, and to yield more readily to the action of the weakest solvents.

I had observed in repeating the experiment of Sultz, that if I wiped my tongue as accurately as possible, the sensation excited by the approach of the two metals in contact was diminished, so as to be hardly distinguished. The saliva, or some other moisture, must therefore be of some importance in this phenomenon. But to ascertain the truth of my supposition, I placed in different goblets filled with water,

1. Separate pieces of metal; for example, in the one gold; in another silver; in a third copper; and in others tin, lead, &c.

2. In other similar goblets I put the same metals as before, but two pieces in each goblet, one of which metals was more, and the other less oxidable. They were separated from contact by a small slip of glass.

3. Lastly, I placed in other goblets metals of different kinds, but two in each, and in immediate contact. The two first sets exhibited no perceptible change, but in the last the most oxidable metal was visibly loaded with oxide, a few moments after having been in contact with a different metal. This oxide gradually increased, so as to hang beyond the lower metal, uniting with it in a mass, and flowing in a cascade along the whole of the sides. This phenomenon commences, though insensibly, at the very instant of contact; but I left the metals of this experiment for a considerable time, to see what would happen. I examined them at the end of a month, and found that the two metals had contracted so considerable an adhesion, that in order to detach a piece of brass about two centimeters broad from a plate of tin, a force of no less than two kilogrammes was necessary.

I afterwards



I afterwards observed, that several metals were not only charged with oxide, but that small saline crystals of different figures were formed. It appeared therefore to me, that an evident chemical action had taken place, and that it was unnecessary to seek farther for the nature of the new stimulus, which in the experiment of Sultzner has been called galvanism. It was manifestly a combustion or oxidation of the metal; the stimulating principle might therefore be either the caloric, which is disengaged, or the oxygen, which passes to new combinations; or lastly, the new metallic salt: but which of these it may be I have not well ascertained. I have in some instances coloured the water, in which I placed the metals with tournsol; but I have remarked no other effect, than a precipitation of that colouring fecula, without any change of its colour. I have remarked, that the water in which this experiment is made, contracts a slight metallic, or as I may say, arsenical taste; which lasts for some time, and produces a disposition to spit; but it does not appear to contain enough of metal to be detected by the most sensible chemical re-agents. I have therefore thought it proper to limit my conclusions respecting this phenomenon, that it is merely a slow combustion of the metal, which must be accompanied with an attraction of oxygen, and a developement of light and caloric. It is well known that when a metal, such for example as gold, is amalgamated with mercury, there is an immediate expulsion of caloric, not perhaps in consequence of the solid state of the mercury, but because the diminution of the force of aggregation in the particles of the latter metal facilitates its combustion. The progressive augmentation of weight which is observable in amalgams, arises only from the oxygen which they attract from the atmosphere. I attempted in vain to measure the quantity of caloric, which is developed by the contact of two solid metals, whatever may be their weight. This quantity is too small and too diffused, as it were, over a large surface, to be ascertained by instruments of so little delicacy as those we possess. Nevertheless, the light which flows from this metallic combustion, may be seen if the eye forms part of the apparatus. For example, it is only requisite to hold a piece of silver in the mouth, and apply a small piece of tin upon the ball of the eye; as soon as these two metals are made to communicate directly, or even by means of a third metal, a faint, but very distinct light is perceived, which is not an electric spark, nor a convulsive irritation. For though this light appears to affect the organ at the first instant only, because the eye soon becomes accustomed to this weak sensation, we may be assured, that the emanation of light is continual in this case, by sliding alternately the transparent and the opaque cornea against the metal, when it will be seen that the light is stronger whenever the metal is touched by the more transparent part of the organ. And again, if this experiment be made as it ought in the dark, nothing more will be necessary, than to attend at the time when the communications between the two metals is interrupted, and the obscurity will be more deep and perfect, which is a proof of the constant presence of some light before that interruption. I do not speak of that kind of flash which some persons affirm they have seen, by applying the two metals simply to the tongue and the gums, without the eye forming part of the circuit. I have not been able to verify this fact on myself, and have remarked, that



several individuals have affirmed, that they saw what others could not perceive, and that the action in all was a convulsive sensation, or delusive appearance, like the light which is seen when the eye is pressed with the finger, or when a blow is given in the vicinity of that organ. It appears, therefore, that the sensation of taste, and the emanation of light, are in this case the results of a chemical operation. But those who have been desirous of attributing the whole to electricity, have not been in want of plausible observations to justify their hypothesis. It has been remarked, for example, that the sensation here described, is felt even when the communication is made by a chain, or a long metallic conductor. But it is known, that electricity is propagated by this means to an indefinite distance; and I have observed, that six or seven metres are the extreme limit to which the manifestation of the metallic action on the tongue or the eye can be extended. It is certainly at the precise point of contact of the two metals, that their mutual action is the strongest, and it is natural to think, that the particles which are the most affected, must communicate from the one to the other in their vicinity, to a certain point, the disposing force which they have received. It must propagate itself with diminished force, like the circles impressed in stagnant water by the fall of a body; and the limit of its action is nearly that which I have pointed out.

By varying my experiments in several manners, I observed, that if I covered the water in which the two metals in contact were placed, with a thin coat of oil, the oxidation was very slight, and stopped at a certain point. But this assuredly did not so happen, because the intervention of a non-conducting fluid had opposed the completion of an electric phenomenon, as it may appear at first sight; for I attempted to keep up the communication of the water and the metals with the common stock, by plunging a metallic conductor beneath the oil, and the combustion was not continued more than before. The same interruption or limitation obtains, if the free contact of the atmosphere be excluded, by means of a small bell glass reversed over mercury, which does not oppose the passage of electricity. Besides which, the Galvanists believe, that their phenomena do not depend on universal electricity, but on the specific electricity, as it is called, of the different metals. If this were the case, it would be difficult to conceal, why the effect should not be produced at the very instant of contact, in the same manner as it happens when two charged bottles of opposite electricities are brought into contact. And again, nothing would prevent the continuation of the phenomenon when the two metals came into contact, whatever might be the circumstances. I am well aware, however, that if it may here perhaps be objected to me, that if the two metals acquire the faculty of decomposing water by their simple contact, and the mere disposition of their attraction, or mutual affinity, a slight coat of oil, or inverted glass, ought not to oppose the continuation of their complete oxidation, while they continue surrounded by that element. I have remarked, that the free contact of the atmosphere is necessary to this phenomenon; because it is necessary that the water should contain that portion of oxygen gas, which it always holds after having remained for a certain time in contact with the air. It is necessary in my apprehension, that

that the hydrogen of the water should be in a state of quartation, or overcharged with oxygen, in the same manner as gold with silver, in order that the solution or parting may take place. The contact of the atmosphere is therefore necessary to the water in the goblet, in order that it may re-assume the necessary state of quartation, by absorbing oxygen gas from the atmosphere, in proportion as the oxidation of the metal seizes the radical of that principle.

If the experiment be made in a calm air, a kind of pellicle may be observed at the surface of the water, immediately over, and of the same figure and size as the metal, beneath which streams even indicate the points of the surface and columns of the water, through which the atmospheric oxygen has been transmitted. This re-absorption is so true, that if in some cases a metallic oxide, well charged with oxygen, be substituted instead of the contact of the atmosphere, the combustion of the metal under experiment is effected with facility. It is known that iron decomposes water without addition, though very slowly; but if the red oxide of lead be added at the bottom of the water, the iron becomes changed into the black oxide without decomposition of the water.

I have obtained, though after a very long time, the oxidation of tin in water in contact with silver, in a bottle of flint glass filled with water, and almost hermetically closed. But I have remarked, that the lead which enters into this kind of glass, yielded its oxygen to the tin, and became changed into a black opaque oxide, in the same manner as happens when a bottle of flint glass, filled with hydrogen gas, is made red hot in the midst of charcoal. The hydrogen is burned, and seizes the oxygen of the lead, which it revives, as the tin operated in the case before-mentioned.

It appears evident, therefore, that the experiment of Sultzzer is nothing more than a combustion or chemical operation, as is proved not only by its result, but its duration. For electricity acts always instantaneously, whereas the effects of the chemical affinities continue as long as the re-agents are not saturated. I left for a very long time in water, pieces of silver wrapped in several folds of tin foil. I took out some of them at different periods, and found the progress of the combustion exactly proportional to the time. In those which I took out the last the tin was corroded, and pierced through all its folds, as if it had been plunged in an acid. But if other proofs be required, to shew that electricity has no part in the phenomenon in question, we might vary the experiments, so as not to hinder the effects of the electric fluid, and evidently to prove by the eyes, that the combustion which happens depends on the disposition of the metals, and their chemical affinity.

For example, if a piece of tin of considerable thickness be placed on the eye, and touched at its opposite surface with a bar of silver, there is no decomposition of water, nor combustion, nor light, and yet the contact of the two metals ought to produce these sensible effects, if they depended on the communication of their electricity. 2. If a piece of tin be held upon the eye, and another piece in the mouth, and a communication be made between them by a bar of silver, there is no more appearance of light than in the former experiment. 3. If a piece of gold be applied on the eye, one of silver on the tongue, and

a com-



a communication be made with an iron key, there is no more appearance of light than in the former instances. 4. Neither does this effect take place if the iron be applied to the eye, and the tin to the tongue, with a communication between them. 5. Gold and silver separately applied to these two organs, give hardly any sensation by their contact. 6. The same event takes place if two pieces of silver be used, and the communication be made by iron. 7. And likewise if copper be placed on the eye, tin on the tongue, and a communication be made by iron. 8. Neither is the sensation more evident if the silver be placed on the eye, the gold on the tongue, and copper be the medium of communication. 9. On the contrary, a considerable light is seen if the iron touches the eye, the silver the tongue, and the copper forms the communication. 10. Or if gold be substituted instead of silver. 11. Or if the communication between the iron on the eye, and the gold on the tongue, be made by means of a silver spatula. 12. Or if the iron on the eye, and the silver on the tongue, communicate directly. 13. Or if the order of these two metals be reversed. 14. Or if gold be used instead of silver. 15. And lastly, the same light of the combustion may be seen, if instead of placing one of the metals on the tongue, both be placed on the eyes.

By these experiments, which are all that I recollect at present, and may very easily be repeated and varied in different manners, we see that it is not electricity which produces the results. For it is well known, that the electric fluid thoroughly and instantaneously penetrates all the metals which most eminently conduct it, whatever may be their situation or relative positions.

But if it be true that the water affords the oxygen to the metal in these cases, it may be demanded, what has become of its hydrogen. It must be immediately observed, that on account of the contact of the atmosphere affording oxygen gas in proportion as the metal consumes it, very little of the water must be decomposed.

I have remarked, that I left the different metals for a long time in contact together, at the end of which I examined them, and I not only found them to be abundantly oxidized, but I also found regular aluminiform saline crystals, adhering particularly to the pieces of silver, and salts of a very determined figure, consisting of two tetrahedral pyramids attached at their base, which appeared to me to be hydrogenated tin.

It is already known that hydrogen dissolves several metals; for in hydrogen itself are found iron, zinc, arsenic, &c. It is known, that the amalgam of zinc and mercury contains hydrogen, which may be driven off by heat.

I must add, that sometimes instead of placing my apparatus of tin and silver in water, I have left it a long time in alcohol, and found parallelepipedic crystals upon the silver, which were very transparent, and appeared to contain copper by their light greenish colour. This copper probably came from the silver; for in general I gave the preference to crown pieces, because I observe, that the irregularities on their surface occasioned by the impression, was greatly favorable to the formation of crystals which lodged in these cavities, and round their edges. I attempted to place the same metals in ammoniac inclosed in a crystal bottle, but without any remarkable effect; probably because the combination of the  
hydrogen



hydrogen is too strong, and the oxygen of the atmosphere could not unite with it in the decomposition of the metal. The ammoniac merely took a slight blueish colour, which shews that it had dissolved a portion of copper from the silver piece.

We see very clearly, from the results which I have obtained from the simple contact of the two metals, namely, the oxide and the saline crystals, that the sensations experienced on the tongue, and on the eye, are the consequence of a chemical operation: and it therefore appears to me, that it is to these new compounds, or their elements, that we are to attribute that mysterious stimulus, which produces convulsive motions of the animal fibre in a great part, at least, of the phenomena of galvanism.

## VI.

### *Report concerning the Art of making fine Cutlery.—W. N.*

THE fabrication of edge tools is one of the first arts among men in every state of society. Artizans are well aware of the necessity, that the instruments of their respective trades should be made to possess the qualities adapted to the operations by which they gain their subsistence; and among the various sub-divisions of labour, there is perhaps no material, upon which the skill and judgement of practical men are more multifariously exercised than steel. The makers of files, of chisels, of planes, saws, and the infinite variety of knives, all occupy their several departments separate from each other, and possess their respective degrees of celebrity among workmen, which are grounded on their knowledge of the peculiar kinds of steel, as well as the methods of working them, which are best suited to the intended operations. Many of these methods are kept secret; but in general the philosophical enquirer will find the communications of operative men, to the full, as liberal and open as the circumstances of the case may seem to warrant. Many manufacturers have no reserve with regard to the manipulations of their art, and have the spirit to assert their claims to public encouragement, upon the open ground of the address and integrity with which they conduct their professional labours.

Among the instances of this kind which have occurred to me during a life of diligent enquiry, I have lately been much gratified by the ready assistance and communications of Mr. Stodart of the Strand, which enable me at present to communicate my own notions on the subject of fine cutlery, with the advantage and support of his successful experience; which I shall proceed to do without farther preface.

It appears to be at present generally agreed, that for all works which do not require welding, cast steel is preferable to any other. For fine cutlery it undoubtedly is. Mr. Stodart uses those bars which are marked Huntsman, but does not suppose it to be of a better quality than that of Walker, and other manufacturers. He complains, that it is much

much worse in quality now than formerly, which complaint I have also heard from other intelligent artists. I did not ask him concerning the art of forging, but take it for granted, that it consists in little more than the acquired skill of managing the bar and the hammer, with the precautions not to injure the texture by strong hammering at too low a heat, or to degrade the quality of the steel, by too much heat or exposure to the current of air from the bellows.

Cutlers do not use any coating to their work at the hardening heat, as the file cutters do; and indeed it seems evidently unnecessary when the article is intended to be tempered and ground. Mr. S. agrees with me, that the best rule is to harden as little as possible above the state intended to be produced by tempering. Work which has been overheated, has a crumbly edge, and will not afford the wire hereafter to be described. The proper heat is a cherry red visible by day-light. He has not found that any advantage is obtained from the use of salt in the water, or cooling that fluid, or from using mercury instead of water; but it may be remarked, that questions respecting the fluid are, properly speaking, applicable only to files, gravers, and such tools as are intended to be left at the extreme of hardness. Yet though Mr. Stodart did not seem to attach much value to peculiarities in the process of hardening, he mentioned it as the observation and practice of one of his workmen, that the charcoal fire should be made up with shavings of leather: and upon being asked, what good he supposed the leather could do, this workman replied, that he could take upon him to say, that he never had had a razor crack in the hardening since he had used this method, though it was a very common accident before.

It appears to me from the consideration of other facts, that this process is likely to prove advantageous. When brittle substances crack in cooling, it always happens from the outside contracting and becoming too small to contain the interior parts. But it is known, that hard steel occupies more space than when soft, and it may easily be inferred, that the nearer the steel approaches to the state of iron, the less will be this increase of dimensions. If then we suppose a razor, or any other piece of steel, to be heated in an open fire with a current of air passing through it, the external part will by the loss of carbon become less steely than before; and when the whole piece comes to be hardened, the inside will be too large for the external part, which will probably crack. But if the piece of steel be wrapped up in the cementing mixture, or if the fire itself contain animal coal, and is put together so as to operate in the manner of that mixture, the external part, instead of being degraded by this heat, will be more carbonated than the internal part, in consequence of which it will be so far from splitting or bursting during its cooling, that it will be acted upon in a contrary direction, tending to render it more dense and solid.

One of the greatest difficulties in hardening steel works of any considerable extent, more especially such articles as are formed of thin plates, or have a variety of parts of different sizes, consists in the apparent impracticability of heating the thicker parts, before the lighter are burned away; besides which, even for a piece of uniform figure, it is no easy matter to make up a fire which shall give a speedy heat, and be nearly of the same intensity throughout.



throughout. This difficulty formed a very considerable impediment to my success in a course of delicate steel work, in which I was engaged about seven years ago; but after various unsuccessful experiments, I succeeded in removing it by the use of a bath of melted lead, which for very justifiable reasons has been kept a secret till now. Pure lead, that is to say, lead containing little or no tin, is ignited to a moderate redness, and then well stirred. Into this the piece is plunged for a few seconds; that is to say, until when brought near the surface that part does not appear less luminous than the rest. The piece is then speedily stirred about in the bath, suddenly drawn out, and plunged into a large mass of water. In this manner a plate of steel may be hardened so as to be perfectly brittle, and yet continue so sound as to ring like a bell; an effect which I never could produce in any other way. Mr. Stodart has lately made trial of this method, and considers it to be a great acquisition to the art, as in fact I found it.

The letting down, or tempering of hard steel, is considered as absolutely necessary for the production of a fine and durable edge. It has been usual to do this by heating the hardened steel, till its bright surface exhibits some known colour by oxidation. The first colour is a very faint straw colour, becoming deeper and deeper by increase of heat, to a fine deep golden yellow, which changes irregularly to purple, then to an uniform blue, succeeded by white and several successive faint repetitions of these series. It is well known, that the hardest state of tempered instruments, such as razors and surgeon's instruments, is indicated by this straw colour; that a deeper colour is required for leather cutter's knives, and other tools that require the edge to be turned on one side; that the blue which indicates a good temper for springs, is almost too soft for any cutting instrument except saws, and such tools as are sharpened with a file; and that the lower states of hardness are not at all adapted to this use. But it is of considerable importance, that the letting down or tempering, as well as the hardening, should be effected by heat equally applied, and that the temperatures, especially at the lower heats, where greater hardness is to be left, should be more precisely ascertained than can be done by the different shades of oxidation. Mr. Hartley first practised the method of immersing hard steel in heated oil, or the fusible compound of lead five parts, tin three, and bismuth eight. The temperature of either of these fluids may be ascertained in the usual manner, when it does not exceed the point at which mercury boils: and by this contrivance the same advantages are obtained in lowering the temperature of an whole instrument, or any number of them at once, as have already been stated in favour of my method of hardening. Oil is preferable to the fusible mixture for several reasons. It is cheaper; it admits of the work being seen during the immersion by reason of its transparency; and there is no occasion for any contrivance to prevent the work from floating.

I requested Mr. Stodart to favour me with an account of the temperatures at which the several colours make their appearance upon hardened steel; in compliance with which he made a series of experiments upon surgeons needles hardened, highly polished, and ex-

posed to a gradual heat while floating at the surface of the fusible mixture. The appearances are as follow :

No. 1. taken out at  $430^{\circ}$  of Fahrenheit. This temperature leaves the steel in the most excellent state for razors and scalpals. The tarnish, or faint yellowish tinge it produces, is too evanescent to be observed without comparison with another piece of polished steel. Instruments in this state retain their edge much longer than those upon which the actual straw colour has been brought, as is the common practice. Mr. S. informs me, that  $430^{\circ}$  is the lowest temperature for letting down, and that the lower degrees will not afford a firm edge.

No. 2 at  $440^{\circ}$ , and 3 at  $450^{\circ}$ . These needles differ so little in their appearance from No. 1, that it is not easy to arrange them with certainty when misplaced.

No. 4 has the evident tinge which workmen call pale straw colour. It was taken out at  $460^{\circ}$ , and has the usual temper of penknives, razors, and other fine edge tools. It is much softer than No. 1, as Mr. Stodart assures me, and this difference exhibits a valuable proof of the advantages of this method of tempering.

Nos. 5, 6, 7 and 8, exhibit successive deeper shades of colour, having been respectively taken out at the temperatures  $470^{\circ}$ ,  $480^{\circ}$ ,  $490^{\circ}$ , and  $500^{\circ}$ . The last is of a bright brownish metallic yellow, very slightly inclining to purple.

No. 9 obtained an uniform deep blue at the temperature of  $580^{\circ}$ . The intermediate shades produced on steel by heats between  $500^{\circ}$  and  $580^{\circ}$  are yellow, brown, red, and purple, which are exhibited irregularly on different parts of the surface. As I had before seen this irregularity, particularly on the surface of a razor of Wootz\*, and had found in my own experience, that the colours on different kinds of steel do not correspond with like degrees of temper, and probably of temperature in their production, I was desirous that some experiments might be made upon it by the same skilful artist. Four beautifully polished blades were therefore exposed to heat on the fusible metal. The first was taken up when it had acquired the fine yellow, or uniform deep straw colour. The second remained on the mixture till the part nearest the stem had become purpleish, at which period a number of small round spots of a purpleish colour appeared in the clear yellow of the blade. The third was left till the thicker parts of the blade were of a deep ruddy purple, but the concave face still continued yellow. This also acquired spots like the other, and a slight cloudiness. These three blades were of cast steel; the fourth, which was made out of a piece called Styrian steel, was left upon the mixture till the red tinge had pervaded almost the whole of its concave face. Two or three spots appeared upon this blade, but the greater part of its surface was variegated with blue clouds, disposed in such a manner as to produce those waving lines which in Damascus steel are called the water. Two results are more immediately suggested by these facts; first, that the irregular production of deep colour upon the surface of brightened steel, may serve to indicate the want of uniformity in its com-

\* On Wootz, see Pearson in the Phil. Transf.



position, as well as the method by an acid which has before been explained in this work\*; and second, that the deep colour being observed to come on first at the thickest parts, Mr. Stodart was disposed to think, that its more speedy appearance was owing to those parts not having been hardened. But upon trial with a plate of steel made quite hard at one end, and left soft at the other, I found that heat applied in the middle produced the regular changes at both ends precisely in the same manner. I suppose, therefore, that the thicker parts sinking deeper into the hot metal, experienced a stronger re-action and better contact, which may have accelerated the communication of heat. It may be here noticed, that we found upon repeating the experiment of applying nitrous acid to bright steel, which was hardened in part only, the black tinge appeared more speedily and strongly upon the hard parts, than the rest of the surface: a remarkable event, for the explanation of which I have no theory to offer.

Let us now suppose our cutting instrument to be forged, hardened, and let down or tempered. It remains to be ground, polished, and set. The grinding of fine cutlery is performed upon a grindstone of a fine close grit, called a Bilson grindstone, and sold at the tool shops in London at a moderate price. The cutlers use water, and do not seem to know any thing of the method by tallow†. The face of the work is rendered finer by subsequent grinding upon mahogany cylinders, with emery of different fineness, or upon cylinders faced with hard pewter, called laps, which are preferable to those with a wooden face. The last polish is given upon a cylinder faced with buff leather, to which crocus, or the red oxide of iron is applied with water. This last operation is attended with considerable danger of heating the work, and almost instantly reducing its temper along the thin edge, which at the same time acquires the colours of oxidation.

The setting now remains to be performed, which is a work of much delicacy and skill: so much so indeed, that Mr. Stodart assures me, he cannot produce the most exquisite and perfect edge if interrupted by conversation, or even by noises in the street. The tool is first whetted upon a hone with oil, by rubbing it backwards and forwards. In all the processes of grinding or wearing down the edge, but more especially in the setting, the artist appears to prefer that stroke which leads the edge according to the action of cutting, instead of making the back run first along the stone. This proceeding is very judicious; for if there be any lump or particle of stone, or other substance lying upon the face of the grinder, and the back of the tool be first run over it, it will proceed beneath the edge, and lift it up, at the same time producing a notch. But on the other hand, if the edge be made to move foremost, and meet such a particle, it will slide beneath it and suffer no injury. Another condition in whetting is, that the hand should not bear heavy: because it is evident, that the same stone must produce a more uniform edge if the steel be worn away by many, than by few strokes. It is also of essential importance, that the hone itself should be of a fine texture, or that its siliceous particles should be very minute. Mr. Stodart informs me, that there are no certain criterions by which an excellent hone can be

\* Philosophical Journal, I. 470.

† Philosophical Journal, I. 131.

distinguished from one of ordinary value, excepting those derived from the actual use of both: that the Turkey stone cuts fast, but is never found with a very fine grit: that the yellow hone is most generally useful, and that any stone of this kind requires to be soaked in oil, and kept wet with that fluid, or otherwise its effects will be the same as that of a coarser stone under the better treatment: and lastly, that there is a green hone found in the old pavement of the streets of London, which is the best material yet known for finishing a fine edge.

The grindstone leaves a ragged edge, which it is the first effect of whetting to reduce to thin, that it may be bended backwards and forwards. This flexible part is called the wire, and if the whetting were to be continued too long, it would break off in pieces without regularity, leaving a finer, though still very imperfect edge, and tending to produce accidents while lying on the face of the stone. The wire is taken off by raising the face of the knife to an angle of about 50 degrees with the surface of the stone, and giving a light stroke edge foremost alternately towards each end of the stone. These strokes produce an edge, the faces of which are inclined to each other in an angle of about 100 degrees, and to which the wire is so slightly adherent, that it may often be taken away entire, and is easily removed, by lightly drawing the edge along the finger nail. The edge thus cleared is generally very even: but it is too thick, and must again be reduced by whetting. A finer wire is by this means produced, which will require to be again taken off, if for want of judgment, or delicacy of hand, the artist should have carried it too far. But we will suppose the obtuse edge to be very even, and the second wire to be scarcely perceptible. In this case the last edge will be very acute, but neither so even nor so strong as to be durably useful.

The finish is given by two or more alternate light strokes with the edge slanting foremost, and the blade of the knife raised, so that its plane forms an angle of about 28 degrees with the face of the stone. This is the angle which by careful observation and measurement, I find Mr. Stodart habitually uses for the finest surgeon's instruments, and which he considers as the best for razors, and other keen cutting tools. The angle of edge is therefore about 56 degrees.

The excellence and uniformity of a fine edge may be ascertained, by its mode of operation when lightly drawn along the surface of the skin, or leather, or any organized soft substance. Lancets are tried by suffering the point to drop gently through a piece of thin soft leather. If the edge be exquisite, it will not only pass with facility, but there will not be the least noise produced, any more than if it had dropped into water. This kind of edge cannot be produced, but by performing the last two or more strokes on the green hone.

The operation of strapping is similar to that of grinding or whetting, and is performed by means of the angular particles of fine crocus, or other material bedded in the face of the strap. It requires less skill than the operation of setting, and is very apt, from the elasticity of the strap, to enlarge the angle of the edge, or round it too much.

*Thoughts,*



## VII.

*Thoughts on Magnetism, by RICHARD KIRWAN, Esq; L. L. D. F. R. S. and M. R. I. A.*

(Concluded from page 94.)

IF the south pole of a magnet be charged with filings of iron, and then approached to a bar of iron standing erect, part of the filings will drop off, because the poles of the same name, by exciting a contrary arrangement, weaken each other; but if the filings were suspended from the north pole of the magnet it would take up still more of the filings, as the opposite poles strengthen each other, the uppermost pole of the iron in this case becoming magnetic by position.

If the synonymous poles of two magnets of unequal powers be approached to each other, if the powers be very unequal the stronger immediately destroys the weaker, and inducing a contrary disposition attracts instead of repelling it; if the powers be less unequal it requires a longer time; so also if one be *softer* than the other. Even if their powers be equal, yet after some time the softer will yield to the harder. If both be equally hard they only *weaken* each other.

If a magnet be cut in two, in a direction parallel to the axis, the parts before conjoined will now repel each other, because they still retain two synonymous poles.

But if the magnet be cut in two in a direction perpendicular to the axis, the two ends before conjoined will now attract each other.

If a magnetic wire be twisted, its powers are so disordered that one side of the wire, in some places of it, will be attracted and the other side repelled by the same pole of the magnet.

The power of the magnets (*cæteris parib.*) is in proportion to their surfaces or as the squares of their diameters.—See *Hutton's Magnetism*, p. 72.

*Communication.*

When iron is applied to or brought within the sphere of activity of a particular magnet, it acquires the arrangement requisite to form the heteronymous pole, and thus becomes itself in some degree magnetic in its whole length, if this length be not totally disproportioned to the power of the particular magnet.

Hence the other end of such bar of iron acquires the the arrangement of the opposite pole, according to the laws of crystalization already laid down.

Iron becomes magnetic either by contact or proximity to a magnet, or by position, or by internal commotion.

If a bar of iron be placed in a vertical position its insensible fibrillæ gradually acquire the magnetic arrangement, so that after some years it becomes a complete magnet, its *lowest* part becoming a north pole, that is, pointing when free to the north, and the *upper* a south pole. In the S. hemisphere the under end becomes a south pole.

A bar

A bar of iron not previously magnetic does not acquire this disposition in the slightest degree while lying in a horizontal or nearly a horizontal disposition, but if one end of it be raised it immediately acquires it in some degree, as appears by approaching a magnetic needle to either end, because in that direction it is then exposed to the activity of the polar ends of the great general magnet.

But if a bar of iron be heated, though only at one end, and while hot set in a vertical or nearly a vertical position, it will acquire the magnetic power much more readily.

So also if one end of a bar of iron not magnetic be struck against the ground it will become in some degree magnetic, the lower end becoming a north pole, &c. and if afterwards the other end be struck in the same manner the poles will be reversed.

Hence it is evident that *any motion* communicated to the integrant particles of iron placed in a proper situation helps them to assume the magnetic disposition already impressed upon them by the great general magnet.

If the opposite poles of two magnets of *equal power* be approached to each other the power of both is increased; and if one of them be *more powerful* than the other it will increase the magnetic disposition, and consequently the power of the weaker.

*Soft* iron, as its parts are most easily moved, receives the magnetic disposition most easily, *hard* iron or tempered steel more difficultly, and *cast* iron, as being both hard and abounding in the heterogeneous particles, most difficultly and imperfectly.

Whatever way iron is applied to a magnet the magnetic power is diffused in the direction of its length. Hence it should seem that when a bar of iron is laid on a magnet the contiguous ends of the iron become poles of the *same name* with those of the magnet to which they are contiguous, and hence may be derived the power of *armed magnets*, for the surfaces of the *armour* immediately beneath those of the magnet impress a direction opposite to their own on those of the magnet, and consequently rectify such surfaces of the magnet as may have been inaccurately directed, and thus strengthen it.

To communicate the magnetic power to iron by friction against a magnet, it is necessary that its pole should slide along the magnet several times in the same direction, for if the directions be alternately opposed the powers received will successively destroy each other.

A synonymous pole is formed at the end at which the friction begins, to that of the magnet applied, and an opposite at that at which it terminates.

#### *Appropriation to Iron.*

It has of old been observed that the magnetic phenomena were peculiar to iron, and the reasons why they are so have been already assigned, but of late some semi-metals have been observed to partake of these properties, as Nickel, Kobalt and Manganese; this has been thought to arise from a mixture of ferruginous particles from which they can be scarcely freed, and with respect to Manganese, and in many cases of the others also, this seems to hold true; but with respect to Nickel, and in some instances of the others also, the magnetic properties they discover seem to me to proceed from their great attraction to  
iron,



iron, particularly when their particles are duly arranged, for then they are exposed to the power of the great general magnet, which acts on them in proportion to this arrangement and their affinity to iron.

*Of Inclination and Declination.*

These phenomena, which are so different in different parts of the globe, and even in different seasons and hours of the day, not being as yet noted with sufficient certainty and precision, I shall for the present decline entering into their explanation.

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VIII.

*On the Fetid Gas of Drains.*

SIR,

*London, May 16, 1800.*

THERE is a phenomenon which occurs too frequently for the comfort and ease of many of the inhabitants of this and other towns which have subterraneous drains, and has not been explained as I can yet find by reading or enquiry. Previous to, or about the time of the late change from fair to frequent and continued rains, the apertures of the common sewers, or great public drains communicating with the streets as well as with houses, privies, &c. emitted a well-known and abominably disgusting smell, which is often perceived upon change of weather. It is also observable in some houses that this smell is always more or less predominant when the windows are shut in at night. Perhaps I may be troubling you upon a very well-known subject, when I request you will explain these facts, and point out the remedy; but I am sure, however, that many of your readers would be glad to have your thoughts upon it, as well as

Your obliged reader,

S. S.

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The subject to which the enquiries of S. S. are directed is, in fact, but little understood. The emission of fetid gas from the drains is found to take place when the barometer falls; and perhaps also upon changes of the thermometer, though I am disposed to think this last is not the case. The offensive gas which thus rises from its subterraneous situation consists, probably, for the most part of sulphurated hydrogen, together with humid effluvia and putrescent matter. Houses which are subject to this infection are remarkable for the speed with which silver plate becomes blackened, even when the presence of the gas is not perceptible to the inhabitants; and they are also characteristically remarkable for disease and mortality.

mortality. I do not know of any experiments which have been strictly instituted to shew that the laws of the elasticity and consequent expansion of the gases under variations of pressure are different, and in what direction and quantity. But it is evident, in the present case, that when the superincumbent pressure of the air above us is diminished, the gas in the drains expands more than the common air near the surface of the earth: so that the former is made to ascend into the apartments of houses and the streets. The action of the fires in dwelling houses causing an ascending current up the chimneys, there must of course be a supply at the doors, windows, and other openings: but when these openings are most effectually shut, as is the case at night, they will less readily supply the air, and a larger portion will be pressed in from the drains. It is certain, nevertheless, that this pressure, as far as it regards the openings into a dwelling house, is in all cases very small. Indeed, there is no probability that it should equal the pressure of half an inch of water. The remedy for this serious and very disgusting inconvenience is, therefore, very easy and simple, and likewise very well-known, though much less so than could be wished when we reflect on the number of houses which are thus incommoded. I do not, therefore, think that the want of novelty ought to be considered as a sufficient reason why I should hesitate to describe for the use of my present correspondent, and others in the like want of information, the very common apparatus which bricklayers call a sink trap.

Let us suppose a drain, or subterraneous gutter, to communicate from beneath the pavement of a kitchen to the principal drain in the street, and that there is a grate, or a set of holes, in the pavement for the purpose of suffering waste water to flow off. These same holes will admit the noxious vapours of the drain into the house. But if any contrivance could be made that the drain should suffer water to run out, and at the same time prevent air from returning back, we should evidently be in possession of a remedy for the subject of complaint. A variety of methods present themselves for the accomplishment of this. The most common is distinguished by the name given above. It consists in inclosing the drain with a flat stone, which serves as a wall, to prevent all communication as to the upper part of the cavity: but the water is suffered to pass off beneath its lower edge by sinking the floor of the drain in that particular part. The depressed part or cavity, therefore, forms a small well or pool, in which water is constantly lodged; and the edge of the stone being designedly low enough to remain constantly immersed, the gas cannot return, unless its pressure be sufficient to sink that surface of the water which is furthest from the house to a depth at least equal to that depression;—an event which never happens.

Though this contrivance is attended with a charge which is very little when counter-balanced against the evils it removes, yet as it requires the pavement to be taken up, and a few other dispositions to be made, there are sufficient motives for preferring still simpler methods if offered. One of these consists in making the cavity or depression of the floor immediately beneath the opening in the pavement, and fixing a piece of wooden or other pipe to pass from the surface of the ground into the stagnant water. It may easily be understood that a perfect closure must be made round the sides of the pipe before the grate



or drain stone is laid down. Another apparatus, which is still simpler in its application is cast for that purpose at some of our iron founderies, or may be made in wood or metal by those in remote situations, who may prefer it. A bowl about six or seven inches in diameter, or even smaller, is cast with a flanch or flat rim, for the purpose of setting it in the pavement. In the bottom, or lowest part of the cavity, there is a round hole, defended by a short upright pipe, through which water, if poured into the bowl, would pass as soon as the bowl is filled to the top of the pipe. A perforated cover, with holes in it, is made so as to fit in the cavity of the bowl; and there is another still smaller bowl rivetted, bottom upwards, to the cover itself. When the cover is put on, the small bowl therefore surrounds the pipe, and when water is poured through the holes, it runs into the larger bowl, and a quantity always remains therein sufficient to stand above the edge of the inverted bowl, because the middle pipe rises higher than that edge. It is scarcely necessary to add, that the waste water will flow through the bowl by means of its pipe; but that the air of the drains can only have access to the inside of the inverted bowl, and cannot enter the house unless its pressure were sufficient to sink the surface of the included water, and raise that in the outer bowl as much as the inverted edge lies lower than the top of the pipe. The pressure is never equal to this quantity.

## IX.

*A Memoir, in which the Question is examined, whether Azote be a simple or compound Body?*

*By CHRISTOPHER GIRTANNER, Doctor of Physic at Gottingen\*.*

THE most celebrated chemists have long been aware of the important part which is performed by azote in all the operations of nature. Lavoisier, Fourcroy, Berthollet, Van Mons, Guiton, Chaptal, Vauquelin, Priestley, Van Marum, Goetting, Wiegler, Von Hauch, Paets Van Troostwyck, Deiman, and many other chemists, have studied the nature of this substance with various degrees of success. To these united labours it is that we are indebted for our knowledge of its singular properties, which are so very different from the properties peculiar to other elastic fluids.

But this singular principle appears to be more especially of importance with regard to its effects in organized bodies.

In the inquiries which I have made for more than twelve years past, on the mechanism of life in animals and plants, I found myself every where stopped by this principle so little known. I saw it appear and disappear in my experiments, without being able to fix it, or to explain the manner in which it had been introduced into the bodies from which I had extracted it. I soon began to suspect that azote was not a simple, but a compound body.

\* Communicated by the author to Citizen Van Mons, and published in the *Annales de Chimie*, XXXIV. 1.

I formed various conjectures, which experiment proved to be false, and had entirely renounced this research in despair, when the dispute which was agitated concerning the azote obtained from the vapour of water again excited my attention. This dispute is not yet terminated, notwithstanding what has been said for and against the change of water into gas by Goetling, Wiegleb, Von Hauch, Westrumb, Achard, Wurzer, Juch, Van Mons, Paets Van Troostwyck, and Dieman. It appears to me that this famous dispute has much analogy with several other controversies mentioned in the annals of our science, and by which the most important points of chemical theory have been fixed. Such was the dispute respecting the existence or non-existence of the carbonic acid in chalk, which engaged all the chemists of Europe; such was the dispute respecting the existence or non-existence of oxygen in the red oxide of mercury: a dispute in which I was myself engaged, and ill-treated by the German chemists, particularly Gren, who was easily put out of temper\*.

Wiegleb, Goetling, Von Crell, maintained:

1. That the vapours of water in their passage through ignited tubes are changed into azote;
2. That this change always happens, and in all circumstances, provided that vapours of the water are brought into contact with red hot bodies;
3. That water is changed into azotic gas by combining with caloric;
4. That water is the ponderable basis of azote gas, and every other gas.
5. That, consequently, the theory of Lavoisier, is false.

The Dutch chemists, as well as Messrs. Von Hauch, Juch, Van Mons, &c. maintained:

1. That the vapours of water in passing through red hot tubes are never, and in no case, converted into azote gas;
2. And that it is only by error that azote gas has been obtained, because it was not a product of water, but part of the atmospheric air which passed through the tubes;
3. That, consequently, the theory of Lavoisier remains unshaken, and the theory of phlogiston, or that of water being the base of all the gases, is erroneous.

I was very much interested in this dispute, of which I very attentively followed the progress. But I was sorry to observe that the spirit of party mixed in the discussion; that harshness prevailed on both sides; that the dispute was not for the acquisition of truth, but victory, and the enquirers had previously decided to find only such facts as their theories required; and, consequently, that their discernment, even in matters of fact, became impaired. Experiments were multiplied; the truth of the narratives was on both sides disputed; results were found absolutely contrary to each other; and the parties,

\* Out of all the opinions which Gren has supported, there is not one in which he has been in the right: but he possesses the merit of having written his *Systematisches Handbuch*, which is an excellent compilation. The hands of Gren were of more value than his head; he compiled well, but his meditations possessed no force. G.



instead of discovering the truth as a common basis of union, became more and more remote from each other in their conclusions.

For my part I was no otherwise interested in this dispute than to support the interests of truth. I am intimately persuaded that the system of Lavoisier is conformable to nature: An experiment is announced to me which is said to overthrow this system totally. Let us see; let us examine; let us repeat; and if the system be false, it is better to abandon it in good time, as not to wait till it abandons us. Let us not be attached to system, but to truth; and when nature speaks, let us listen to her voice in preference to Stahl or Lavoisier, Descartes or Newton. Whatever may be the result of our experiments, we shall not fail to profit. The thing of which we risk the loss being merely error, we cannot be too desirous of undergoing it.

Thus it was that I reasoned. I had learned by the history of chemistry that in all disputes where the parties obtained opposite results from the same experiments, there is an error in the manner of expressing themselves, and that at the bottom both parties are in the right. I was very much disposed to think that the case before us was a question of this kind.

I, therefore, proposed to myself the resolution of the following questions:

1. Are the vapours of boiling water changed into azote gas in their passage through ignited tubes?

2. In what circumstances does this change take place?

3. What is the cause of this production of azote gas?

4. Are the experiments contrary to the system of Lavoisier, or not?

I must first confess, before I enter upon this discussion, that the manner in which the production of azote gas had been explained, in supposing the external air to pass through the tubes, did not appear to me at all satisfactory.

I had myself formerly adopted this explanation\*; but I very soon gave up an opinion so contrary to every thing we know of chemistry.

I am really concerned to see this improbable opinion maintained by chemists of the first rank. But the history of chemistry supplies us with many examples of the same nature. Before the immortal Lavoisier had proscribed the doctrine of phlogiston, the Stahlans removed a leading difficulty of their system in a manner absolutely similar. The oxide of mercury was put into a crucible, which was closed in the most accurate manner, and then exposed to heat. Upon opening it after cooling, it was found to contain running mercury. Now a reduction of this kind not being possible, according to the doctrine of Stahl, without the intermedium of phlogiston, the partizans of that doctrine were asked for an explanation of a fact so contrary to it. They replied, that the phlogiston had passed through the crucible to unite with the mercury. Even Bergmann and Scheele were content with so absurd an explanation†, which proves that the spirit of system misleads the best understandings, and renders them ridiculous in the eyes of posterity.

\* *Anfangsgrunde der Antiphlogistischen Chemie*, second edition, page 89---90.

† Scheele *Von luft und feuer*, edition of Leonhardi, page 42.

less prejudiced and more enlightened. I shall shew that azote does not pass through ignited tubes, any more than phlogiston passes through crucibles. The experiments which Priestley has made to prove that air passes through earthen retorts do in no respect prove it. He saw the retorts smoke on their outer surface: but this vapour was not, as he imagined, water passing through: but the external surface of the earthen retort attracted water from the atmosphere.

(To be continued.)

ms. b.

## X.

*On the Power of the Oxygenated Muriatic Acid in Vegetation. By a Correspondent.*

TO MR. NICHOLSON.

SIR,

FROM a conviction that you have a satisfaction in giving a place in your useful Journal, to any hints that tend to restore an important discovery to its rightful owner, I am induced to transmit to you the annexed transcript of part of an essay written by the late ingenious Dr. Ingenhoufz.

It was published in the year 1796, in an appendix to the general report of the Board of Agriculture: and clearly demonstrates, that at that period, the Doctor not only possessed an idea of the power of the oxygenated muriatic acid in accelerating the germination of seeds, but had actually put it to the test of experiment. I need hardly add, that the effects of this acid in refuscitating the dormant powers of vegetable animation, has lately been imported as a philosophical novelty from the Continent.

"As I have made mention more than once in this paper, of a letter I wrote to Sir John Sinclair, dated Dec. 2d. 1794, on different articles relating to agriculture, and among others, upon the beneficial effects of alkaline salts in promoting vegetation, and respecting the effects of some other salts; and as that letter makes no part of this paper, I think it proper to inform the reader, that the particularly good effect of alkaline salt was so manifest in my own garden, that all the gardeners who saw it thought it equal to the effects of the best horse dung. I repeated the application of that salt last year (1795) at Hartford with the Hon. Baron N. Dimsdale, M. P. in his garden, and that gentleman was equally convinced as myself, of its manifest good effects. We tried at the same time, the application of different neutral salts, the particulars of which experiments I may possibly publish on some future occasion. We made also many experiments with different solutions, and medicated liquors poured upon the ground, as well as steeping the seeds of different grains in them. Be it sufficient to say here, that of all the neutral salts we tried, the glauber salt did seem to be one of the best in promoting vegetation; and that the steeping the seeds in the oxygenated marine or muriatic acid, (which is now much employed in bleaching linen in



in an expeditious way) had a particularly beneficial effect in producing early and vigorous plants. The beneficial effects of these different substances may be easily accounted for by an intelligent reader, according to the theory laid down in this paper."

"We were somewhat astonished that those seeds, *viz.* wheat, barley, rye, and oats, which had been steeped in the above-mentioned oxygenated muriatic liquid, even during forty-eight hours, did thrive admirably well; whereas the same seeds steeped during so long a time in some of the other medicated liquids, were much hurt, or had lost their vegetative power. The same oxygenated liquid poured upon the ground had also a beneficial effect."

The dissertation from which the above extract is made, although evidently a hasty and unfinished production of its truly scientific and respectable author, contains so many original hints, and useful views, respecting the important object of increasing the subsistence of mankind, as derived from the production of farinaceous vegetable matter, that it is to be regretted, it is not known beyond the circle to which its peculiar mode of publication has necessarily limited it.

Your's

A constant reader,

B.

### SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

*An Account of the Irides or Coronæ, which appear around and contiguous to the Bodies of the Sun, Moon, and other luminous Objects* \*. Octavo, 46 p. one Plate. Cadell and Davies. 1799.

THIS work, as well as the other concerning the Inflexions of Light, noticed in the Journal of the last month, page 78. is the production of Gibbes Walker Jordan, Esq. the initials alone of whose name are affixed to the concluding page of each work. This contains an application of some of the principles and discoveries contained in that, to the explanation of the phenomena named in the title, an explanation which how greatly sooner it might have been hitherto desired, was certainly unattainable before those discoveries were known.

All irides, or coronæ, our author divides into four sorts; 1. irides consisting of many concentric orders of colours contiguous to the sun or moon; 2. the iris of 45 degrees diameter; 3. the iris of about 84°; and 4. the iris of about 100 degrees diameter, the primary and secondary rainbows of philosophy. After observing the insufficiency of the principles applied by Des Cartes, Huygens, and Newton, to the explanation of these dif-

\* For this article I am obliged to the same correspondent as for the former.

ferent appearances, and reprobating the construction of frozen machineries for the purpose, he proceeds to state the circumstances under which the irides of the first fort, the objects of immediate enquiry, are exhibited.

These irides appear of one or more concentric orders and circles of colours, close around and contiguous to the sun or moon, and sometimes around the brightest of the planets and fixed stars. The colours from the luminary outwards are greyish, black or blue, or faint white, succeeded by a broad dense white, followed by yellow, then by red; then by violet, blue, green, yellow, red; green, diluted yellow, red; diluted green, diluted red. The second order is frequently only green, yellow, red; and the third only diluted green, red. The two first orders are most frequently seen, the third less frequently, the fourth most rarely. The first may be seen every night, when the moon, with more than half a face, shines through thin white fleecy clouds, and through similar clouds by receiving the sun's image through a small hole into a darkened chamber on a sheet of white paper:

Of these irides the diameters vary considerably: that of the first order from one degree to  $5\frac{1}{2}$ , and that of the second from  $3\frac{1}{2}$  to  $10\frac{1}{2}$  degrees. The general breadth of the first order of colours is rather more than 45 minutes, and those of the others are successively less.

In explaining these phænomena, icy crystalizations of all sorts are rejected, and globules of water, the only regular forms of concretion which the vapours of the atmosphere admit of, are alone to be resorted to for the principles of existence of regular and constant phænomena.

For explaining these phænomena as produced by globules of water, all the hitherto accepted principles are shewn to be insufficient. The principle of parallel, or efficient rays, employed in the schools to account for the exhibition of the primary and secondary rainbows, is exceptionable in its former use, and is not here to be received, because vision of images is not by parallel rays, because the light after passage, if sufficiently strong to be seen, would only produce a broad circle round the luminary of decaying white light.

Newton explains these phænomena by the fits of easy transmission and reflection, and by comparing the refraction of the rays passing through a drop of water, to the reflection of a slender beam of light from the back part of a glass lens. The fits of easy transmission and reflection have no existence. The circumstances of the refracted light in the phænomena are altogether, and essentially different from those of the light in the experiment by reflection from a lens, and it is further shewn to be impossible by any refractions through a drop to produce appearances of the observed orders of colours. It is a principle derived from the doctrine of images, and used in explaining the primary and secondary rainbows, that the colours of the drop are inverted in the bow; and according to this principle, these irides, if formed by refractions through the drops, would have their blues external, their reds internal, contrary to observation. For this same reason, and from an essential change in the attendant circumstances, these phænomena cannot be produced by the inflections observed in a slender beam of light passing by the edge of a single body.



The only true principles of explanation of these phenomena, are to be found among those new observations concerning the inflections of light before spoken of.

When two bodies approach one another in a solar beam, the passing light between their edges is divided and distributed on both sides into two complete sets of fringes. *Vide Journal, Observation ε, page 81.* Each of these sets of fringes beginning from the central distance between the bodies, and advancing towards either of the bodies, is composed of the same colours and orders of colours as the observed irides. Of these the blues are least, the reds most bent, the blues furthest from, the reds nearest to the body.

Whenever therefore two globules of water in a cloud or vapour approach near each other, the light passing between them will be distributed into similar fringes, and the union of these fringes from between different drops, will produce all around the luminary circles, and orders of colours, such as by observation are really found to be exhibited.

The nearer to, or more remote the two bodies of the experiment are from each other, the more or less broad will be the fringes, and therefore the breadths also of the irides will depend upon, and vary with the distances of the globules, and thence the observed variations of breadth are easily accounted for. The breadths are varied also, and more especially in vapours of considerable extent or thickness, by the actions of the succeeding series of globules, for although not in the same plane, bodies, and consequently these globules, act upon the light passing between them, and form fringes. *Vide Journal, Observation η, page 81.*

These principles being applied to explain the various phenomena of lights and coloured circles, exhibited in various situations by the sun and moon through the vapours of the atmosphere, they are next extended to all similar phenomena observed round other luminous objects as well as these. The flames of candles and lamps seen through steam, through the exhausted receiver, through thin flocks of wool or cotton, through stuffs of cotton, linen, silk, or wool, through the glasses of coach windows, on which small drops of water are precipitated from the breath of the inclosed persons, are surrounded with coronæ produced in a similar manner. Tapers are seen to burn blue by an eye charged with semipellucid humours arising from injury, watchfulness, and other causes producing rings of colours, sometimes one of dusky blueish green, faintly terminated with yellow and red; and sometimes a second of dusky green and red. A phenomenon of this sort occurred to Des Cartes. A hole in a window shutter will also, under similar circumstances, produce similar appearances.

These observations are illustrated by figures of reference, and may further be confirmed by viewing a candle at the distance of ten feet or more through a thin flock of wool, or by breathing on a pane of window-glass, and looking at the image of a candle reflected obliquely from the glass through the precipitated drops of moisture, when a single halo, composed of the first order of colours terminated with yellow and red, will be distinctly seen.

All the circumstances of these phænomena being thus fully considered and explained, and no doubt remaining of their characters and causes, they may be called *Irides by Inflection*.

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*A general View of the Nature and Objects of Chemistry, and of its Nature and Application to Arts and Manufactures. By William Henry, Member of the Royal Medical, and Natural History Societies of Edinburgh; of the Chemical Society of Jena in Saxony: and of the Literary and Philosophical Society of Manchester. Octavo, 44 Pages. Johnson. London.*

It is usual for those philosophers who undertake to explain the several departments of science by lectures, to publish a syllabus, or outline of their subject in the order according to which they propose to treat it. These short works, if drawn out with ability, are not only of eminent use to the classes who may attend the lecturer, but also to the community at large, as exhibitions of the state of science, and admirable helps to the memory. Some of these works have the form of a systematical enumeration: others that of a regular discourse. The present work is of the latter kind, and presents an elegant and comprehensive view of the objects, nature, and application, of chemical science.

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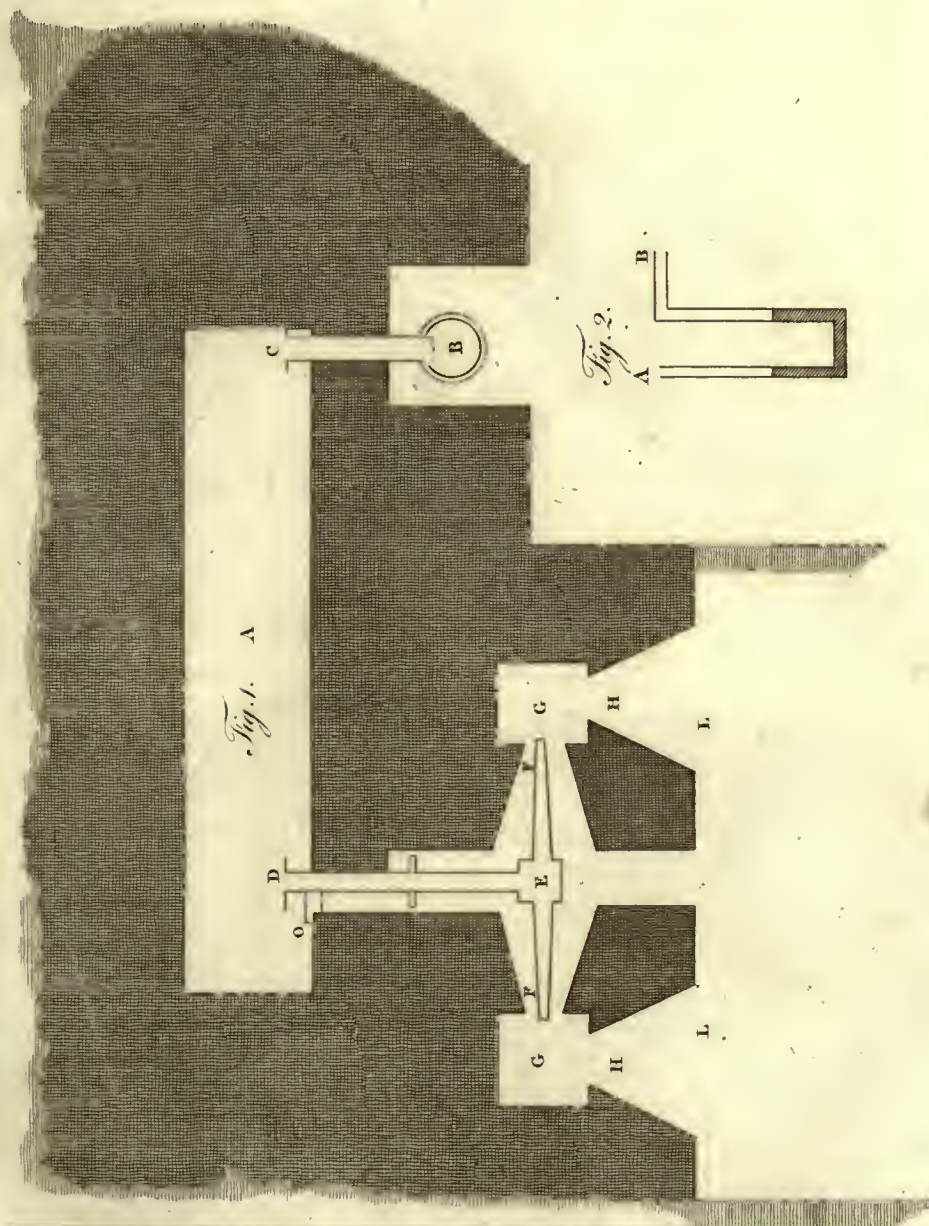
*Description d'un Télégraphe très simple et à la portée de tout le Monde, avec une Planche. A Paris, chez l'auteur, rue de la Liberté, No. 83. Pluviose An. 8. de l'imprimerie de l'Institut des aveugles-travailleurs, 16 Pages in 8vo. Prix 8 sols.—Or, a Description of a very simple Telegraph, practicable by any Person: with an Engraving.*

The telegraph proposed by this author is the human body: the arms of which are capable of forming with regard to each other, as well as to the perpendicular line of the trunk, a great number of figures, sufficiently distinct to be easily seen at considerable distances by the naked eye, or with a telescope. The author develops his method, and the means of carrying it into effect: and he thinks, that in this manner lines of moveable telegraphs may be formed, which may become very useful in war, to keep up a speedy and constant communication between the different bodies of an army, or between the fixed telegraphs of other constructions.

This memoir is part of a larger work on the present and other subjects connected it, which will hereafter appear.

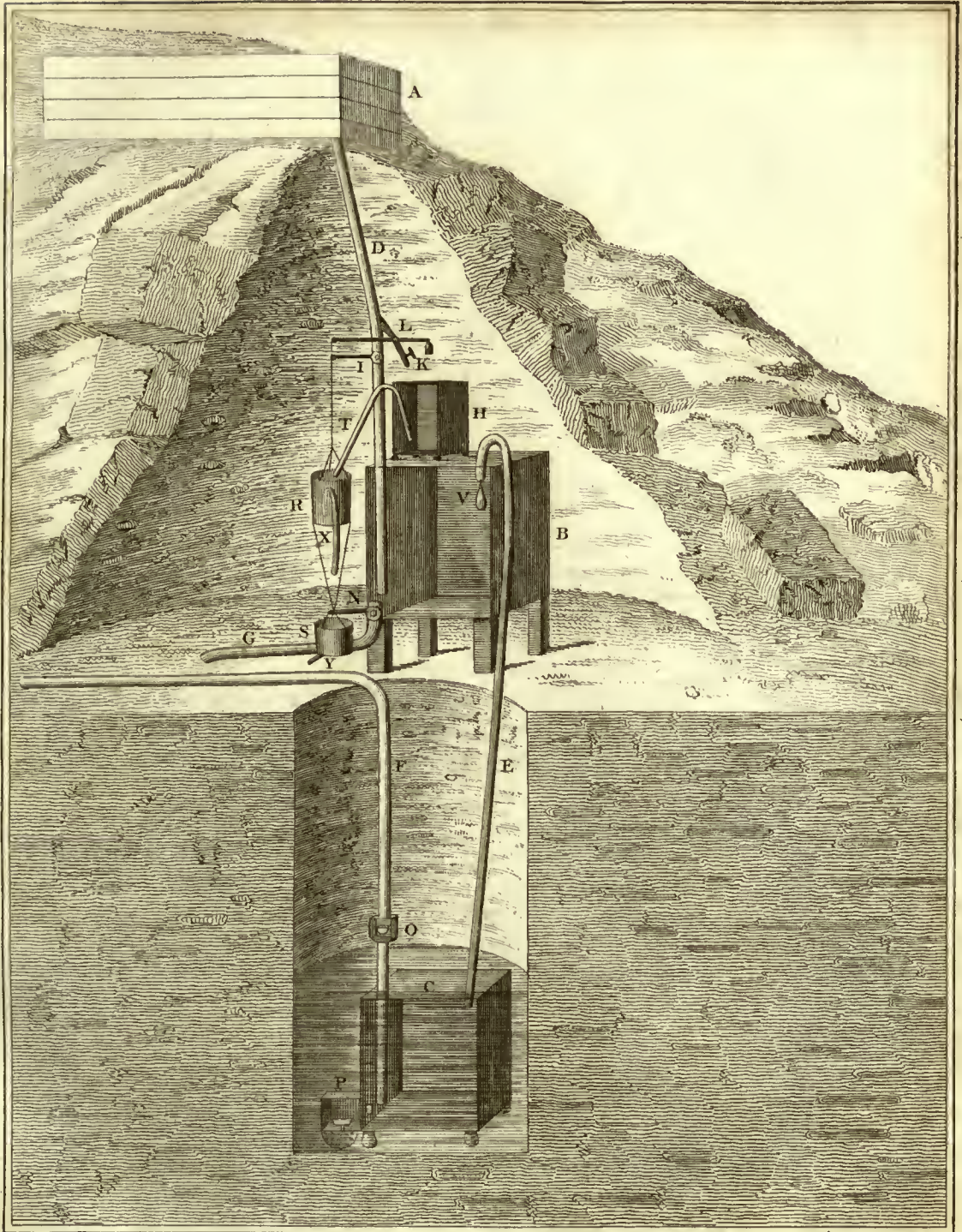
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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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JULY 1800.

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ARTICLE I.

*Description of a new Method of extracting Silver from Copper-Mat by means of Lead, by which the Eliquation of Black-Copper is rendered unnecessary. By the late Dr. GREN\*, Prof. at Halle in Saxony.*

NOTWITHSTANDING the considerable progress which the metallurgical part of scientific chemistry has made in modern times, it must be allowed, that its influence on the manipulations in the smelting works has hitherto been but of little importance, and that many of these operations are carried on by the processes used several centuries ago; with all the errors which have been discovered by subsequent improvements in chemical knowledge. The introduction of the *present process of eliquation*, was indeed a material and great improvement on the former method of separating silver from copper, by which the *copper-mat* † was combined with lead, and the mass afterwards subjected to gradual heat. For in this

\* Translated from Dr. Scherer's, successor to Gren in the professorship at Halle, *General Chemical Journal*, Vol. IV. page 155.

† *Copper-mat* (Kupfer-rohstein) is the product obtained from pyritous copper-ores, having undergone the first or *crude fusion*, by which the stony matrix only has been separated.---Gren's *Principles of Modern Chemistry*, § 1444.---Transl.

method, the requisite or precise proportion of lead to copper, was totally unknown. As ingenious, however, as the modern eliquating process may appear in a chemical view, it is still in its own nature attended with many considerable imperfections; namely, a great loss of time, of fuel, and especially of lead. These imperfections are inseparable from this process, and render it so very expensive, as to be totally inapplicable in the operations with *black copper*\*, which contains less than four ounces of silver. It is well known that *Born* has recommended the amalgamation of the copper-mat as a more advantageous method to extract its silver. But even though the amalgamating process, of which the efficacy and utility is manifest in the richer silver ores, had not been proved by experience to be totally impracticable with respect to such products as contain less silver, for example the copper-mat; yet the operation is rendered precarious, and its establishment becomes an object of risque from the circumstance, that the material requisite, is in the hands of a possessor, who may raise its price at his own arbitrary choice.

My researches into the copper-mat, and its constituent parts, as well as a more accurate ætiology of the whole of the usual process, of extracting from it the copper and silver in a separate state; together with the inquiries into the mutual powers of chemical attraction of the sulphur and the metals contained in it, have suggested to me a new and profitable method of separating the silver from copper-mat by means of lead, by which the whole process of eliquating the black copper is rendered unnecessary. In this method, not only the loss of lead by oxidation, which renders the eliquating process so expensive, but likewise all the other charges of this process are avoided; as for instance, those of the building, and the requisite furnaces and edifices, the necessary fuel, &c. My method is no less simple than easy; it is certain and effectual; and (what principally recommends it) it requires no particular edifice; but only a slight alteration in the form of the usual furnace with a concave floor (*Krummofen*). It is founded on invariable chemical, hydrostatic principles; and is found to be practically applicable and effectual in all the experiments which I have repeatedly made in the small way, and which, in this case, cannot but be the same as those in the large way.

*Principles, on which the new Method is grounded.*

1. Copper-mat consists of sulphur, copper, iron, and silver; so that these metals are completely dissolved by the sulphur.
2. Sulphur can dissolve in fusion only a determinate quantity of reguline metals previous to its saturation.
3. Sulphur has a stronger chemical attraction for iron, than for copper, lead, and silver.
4. Sulphur likewise attracts copper more strongly, than lead and silver.
5. And lastly, the chemical affinity of sulphur with lead is greater, than its affinity with

\* *Black copper* is the metal, which is obtained by a second fusion from copper ores.---Transl.



silver. Hence the graduated arrangement of the chemical attractions of sulphur, with respect to the metals mentioned, is as follows :

SULPHUR,  
IRON,  
COPPER,

LEAD,  
SILVER.

6. When the sulphur is saturated with copper, it can no longer dissolve any portion of lead and silver.

7. Silver and lead have a very strong chemical attraction for each other.

*Inferences from these fundamental Principles.*

From these assertions, unquestionably proved by experience, it follows, that if *copper-mat*, the sulphur of which (the proportion having been so much altered by roasting, as to be saturated with copper, is made to be penetrated by lead in fusion, the lead must then take up all the silver which it contains, and in such a manner, that no part of the lead can be dissolved by the sulphur contained in the copper-mat.

But it would be very wrong to conclude, that the silver can be completely extracted from this roasted copper-mat, merely by subjecting the lead, together with the copper-mat, to a melting heat. For the lead, on account of its easier fusibility, would melt sooner than the copper-mat, sink down to the hearth of the furnace, and by its greater specific gravity always remain beneath the fused copper-mat; and consequently it would deprive this last of its silver merely at the surface, with which it is in contact, and no farther. If, therefore, it be wished to obtain the intended object, it will be necessary that the fluid copper-mat should be forced to rise through the lead in fusion, and to penetrate it entirely.

It is on this circumstance, that the characteristic and most essential part of my new method depends.

It consists in the management, by which the copper-mat (duly roasted, and in which the sulphur is saturated with the copper) is made to rise immediately, and during its fusion through the melted lead. I might call this new method an *amalgamation of fused copper-mat with fused lead*.

The possibility of effecting the conditions, requisite for this purpose, appears very easily from *hydrostatical* principles.—Let the space of the outward floor, or hearth B, Fig. 1. Plate VIII. be connected with the cavity of the furnace A, by means of a hole (a). Now if there be only melted lead in the furnace, it will stand on the outward hearth, equally as high as in the furnace itself. Suppose its horizontal surface to be as marked (bc); then if the melted copper-mat in the interior furnace A, which is of less specific gravity, be above that height, it will rise by the laws of the *equilibrium* in the outward hearth B.

The second chief condition is obtained, by an easy management of the fire employed to effect the fusion. This melting furnace has a recurved bottom, and is provided with an outward hearth, (*Vorderbeerd*) and a draining hearth (*Stichbeerd*). Its essential form of construction is sufficiently shewn in the drawing, before referred to. In this manner the

hearth of the furnace lies partly within and partly without; but it is not necessary, that the projecting part of its hearth should be of considerable breadth. The aperture (a), by which the exterior part of the furnace communicates with the interior, should be placed immediately above the floor or bottom of the hearth, or which is still better in that bottom itself. And the outer hearth of the furnace should have an incision with a gutter, through which the melted copper-mat, rising above the lead flows off continually, and is collected in a pot (vortiegel) placed on the floor of the smelting-house. There is, besides, on the side of the outward hearth, another adjoining hearth, into which the lead of the former, when sufficiently impregnated with silver may be drawn off, to be afterwards put into iron vessels. The most convenient and advantageous dimensions of those hearths, especially the height of the outward-hearth from the floor up to the twyer of the bellows, can indeed be ascertained by experiments only, but these may be easily made without much expence.

In order to deprive the copper-mat of its silver by means of lead in this apparatus of the furnace, and to manage the fusion properly, it is required that the proportion of the specific gravity of the copper-mat to that of the lead should be known. For, as this process is grounded on the well known hydrostatical principle, that the heights of two fluids of different kinds, placed in connected tubes, if required to be in *equilibrio*, must be to each other in the inverse ratio of their specific gravities, it is evident, that the melted copper-mat in the furnace, to keep the equilibrium with the fused lead in the outward hearth, must form a column proportionably so much higher, as it is exceeded by the lead in specific gravity. It is therefore from the specific gravities of both these substances, that we may find how far distant the twyer must be from the lowest point of the hearth, and to what height the lead may be suffered to rise in the outer hearth, with causing the fluid copper-mat to enter into that aperture. The specific gravity of copper-mat is variable, according to the different degrees of its roasting. That of fused copper-mat varies from 4.66 after the second roasting, to 5.20 after the third roasting.

But for the sake of greater security, the least weight only should in this case be assumed as a standard. The specific gravity of lead, at a mean rate, is 11.35.

From these data it is obvious, that if the height of the *lead* in the outward hearth be *six* inches, the *copper-mat* in the interior furnace, to be equiponderant, must stand *fourteen inches and one half* high.—If then the copper-mat, rising through the lead in the outward hearth, covers the surface of this last to the height of  $\frac{1}{2}$  an inch, its column must likewise be proportionally higher in the furnace, and consequently reach to 16 inches. It is therefore evident, that, when the lead rises to 6 inches height in the outward hearth, the distance of the twyer, or nozzle of the bellows, must amount to at least 18 or 20 inches from the lowest point of the hearth.

According to the above stated specific gravity of lead, one Rhinland cubic \* foot of that metal weighs 750 lb. Cologne weight. When therefore 300 pounds of lead are employed

\* The Rhinland foot is reckoned at 12,396 English inches, and the mark of Cologne is 3612 grains, whence the pound will be 7224 grains, or about half an ounce more than our avoirdupois pound.---N.



in this operation, their volume will amount to 760 cubic Rhinland inches; and hence if the perpendicular height of these three centners of lead shall be 6 inches in the outward hearth, it follows, that the breadth of this hearth must be 9 inches, and its length a little more than 14 inches.

In performing the process, the furnace is previously heated to a sufficient degree, and the lead afterwards introduced on the outer hearth; because the heat is there sufficiently intense to fuse it. Its surface is secured against oxidation, by covering it with powdered charcoal. At the same time the furnace is charged with copper-mat, and the requisite quantity of coals, and the fusion effected by a clear fire. As the operation proceeds the lower part of the copper-mat issuing from the furnace through the aperture (*a*), and rising through the body of the lead to its surface, is farther prevented from cooling, by means of some pieces of burning wood. The best and most useful proportion of the coal, to that of the copper-mat in this fusion, must be determined by experience.

The copper-mat being now deprived of its portion of silver, and flowing into the fore-pot (*vortiegel*) from above the lead in the outer hearth, is once more roasted, previous to extracting the copper. But it is to be observed, that by this method of divesting it of its silver, it at the same time undergoes a farther roasting; on which account another part of the expence is saved in my process.

In the management of this process, it is evident, that as much copper-mat as may be thought fit, can be decomposed by the same individual quantity of lead; and accordingly, that this last may be impregnated with silver at pleasure in various degrees. It is not advisable, however, to alloy the lead with too great a proportion of silver; because a small loss of the compound metal would then produce a more considerable loss of silver. But I must leave this to the judgment of the artist, and to future experience.

When the lead is to be run off, the receiver must be first duly heated, and then the tap hole opened. The whole of the lead is thus drawn off from the outer hearth of the furnace, and its surface covered with charcoal-dust. Fresh lead may then be again conveyed, after stopping the tap-hole, into the outer hearth, and the operation continued as before.

Whenever the furnace requires to be cooled by the blast of the bellows, the *work lead*\* is first entirely drawn off from the outer hearth, and also laded out of the draining hearth; after which, all the copper-mat that remains in the outward hearth, and in the furnace, is suffered to run into the draining hearth, now cleared of the lead. But this copper-mat, not being yet deprived of the silver it contains, must be added at the next charge of the furnace.

The lead, which is thus alloyed with silver, is at last subjected to cupellation in the usual way. The *litharge*, which is obtained in this operation, the metal absorbed in by

\* *Work-lead*, or lead freed from copper, and mixed with silver by the operation. See *Gren's Principles of Chemistry*, § 1478, 1486.---Transl.

the *teft* (heerd) and the *first waste* \* (abzug) need not be reduced to the reguline state by a separate operation ; because this may be done at the same time, that another portion of copper-mat is cleared of its silver by fusing them together with it. But as this perfection of any art can only be obtained by experience, and dispatch in the manipulations can be acquired by practice alone, it will be found necessary, even in this instance, to perform repeated experiments, for the purpose of ascertaining the best dimensions of the parts of the furnace, the most suitable proportion of the quantity of coals to be employed, and the most advantageous direction of the bellows pipe ; as well as the most convenient regulation of the blast, and the most suitable proportion of the copper-mat to the lead to be employed in this process.

The advantages afforded by my method, compared with those resulting from the process of eliquation, are evident. No lead is here lost by combustion, because it is accurately covered by the copper-mat, and no intense heat is required, because these substances are of easy fusion. A much smaller quantity of lead is also sufficient in this process, than in that of eliquation ; perhaps only a fifth part, or still less ; whence, upon the whole, the loss or waste of lead, including the cupellation, is considerably less. Besides this, my process requires but little expence in the building or apparatus, and the expences of smelting are by this means in part compensated ; several fires for roasting the copper-mat being rendered unnecessary, and thus saved. From this diminution of expences in my new method, such copper-mats may likewise be deprived of their silver, as when worked into black copper are so poor in silver, as not to repay the charges of eliquation.

By these means, therefore, when the quantity of fused copper-mat in the furnace A has sufficiently increased, it will reach the communicating aperture (a), after having driven before it all the lead into the outward-hearth. Lastly, as soon as the copper-mat has advanced to beneath the surface of the lead in the outward-hearth B, it will rise through the lead by the laws of hydrostatics, and penetrating its mass, will place itself on the surface of the lead, continually accumulating there, until having reached the proper height, it can run off through the indentation cut in the rim of the fore-hearth.

In this simple manner, therefore, any required quantity of copper-mat may, by repeated charging of the furnace, pass through a determined quantity of lead ; and may be thus deprived of its silver without loss in burned lead, and the lead itself may at pleasure be more or less impregnated with silver.

*Experiments in the small Way, to confirm the Success of this Method.*

In order to perform these operations in the small way, as well to confirm the theory and practice, and at the same time to discover a method of making the necessary experiments which might ensure correspondent performance in the large way, I proceeded in the following manner :

\* *First waste* is the dross or scoria formed in the first part of the operation, when the cupelling is performed in the large way in the refining furnace. See Gren's Principles of Chem. §. 1486.---Transl.



A Hessian crucible must be perforated at the bottom, and both its internal and external surfaces coated with charcoal-dust and loam. This crucible is then placed in another calice-formed crucible (*kelchtutte*), or into a black-lead crucible, properly cut for that purpose, and likewise internally coated with powdered charcoal. In this it is fastened by means of a wedge made of clay, in such a manner that it cannot elevate itself during the experiment. A determinate quantity of lead, altogether free from silver, is then fused in the interior crucible, through the aperture of which in the bottom it flows into the exterior vessel. The quantity of lead should be sufficient to occupy part of the inner crucible. And to protect it against oxydation, its surface must be covered with some powdered charcoal in both vessels. Upon this some powdered copper-mat, that has undergone the second or third roasting, is introduced into the inner crucible, and melted by a fire sufficiently strong and brisk, so that it may flow thin. The putting in of fresh copper-mat is to be continued, till the interior crucible can hold no more, when in thin fusion. The fluid copper-mat presses the fused lead in the inner crucible, through its aperture at bottom into the outer, and at last itself follows, and rises up through the lead, and giving out its portion of silver during its transition. That portion only of copper-mat which remains in the inner vessel will continue to preserve its silver. The apparatus is then suffered to become cold without agitation, and after breaking the vessels, the whole of the lead and copper-mat resting upon it in the outer crucible are to be collected, separating them carefully from each other, and cleaning them from the adhering charcoal-dust. Their weights are next to be exactly determined; after which the *lead* is assayed for silver, and the *copper-mat*, deprived of its silver, is assayed for copper. A calculation may be made from the results of the quantity of silver obtainable from a certain determinate quantity of copper.

If the copper-mat has not been duly roasted, and, consequently, still contains too much sulphur, it will dissolve a portion of silver in this process, and a loss will be observed in the remaining lead, which will be greater in proportion as the sulphur in the copper-mat is less saturated with copper. But if the copper-mat, by too much roasting, has been brought below the point of saturation of the sulphur with the copper, part of the copper will then separate during the fusion, and, mixing with the lead, will occasion an increase of weight.

On making experiments with the copper-mat from *Rothenburg*, I found that a mixture of that which had sustained the *third* roasting heat, with an equal portion of that which had been subjected to the *second* roasting, was the best suited, and of such a quality, that the effect was quite satisfactory.

Out of many different experiments, which I have made in order to put my method to trial, and which afforded very uniform results, I shall in this place give an account only of the last I have made.

Copper-mat from *Rothenburg*, mixed in equal parts of that of the second with that of the third roasting, was made to pass, in the manner above explained, through 24 centners:  
of

of lead. The lead was again recovered with an increase of weight, not yet fully amounting to 2 per cent. The *work-lead* contained in the centner *two* ounces,  $1\frac{1}{8}\frac{7}{8}$  drachms of silver, and hence in the whole 51 ounces, and one and  $\frac{1}{8}$  drachm. The whole quantity of the copper-mat, divested of its silver, in this operation, by means of lead, amounted to 9 centners of black copper; hence one centner of black copper at 110 lb. yielded 5 ounces, 5 and  $\frac{6}{11}\frac{5}{11}$  drachms of silver. When the copper, obtained from this copper-mat, was examined, it was found to contain no more than  $3\frac{1}{8}$  drachms of silver, and, therefore, less than is usually the case with eliquated copper.

*A more minute Explanation of this Process, as performed in the large Way.*

Two conditions are essentially requisite to the management of this process. (1) The copper-mat must have been roasted in the due degree. (2) The melting-furnace must have been so constructed, that the copper-mat, while in fusion, may be enabled to pass through the body of the melted lead.

The necessity of the first condition is evident from what has been already said; namely, that when the copper-mat still contains too large a portion of sulphur not saturated with that metal, part of the lead will be dissolved by the sulphur during the fusion, and an irrecoverable loss will be thus occasioned. On the contrary, when the copper-mat has been too strongly roasted, it then no longer contains a quantity of sulphur sufficient to hold all the copper in solution. Part of the latter will, therefore, separate, and unite with the lead. But it is better to fall into the last error than the first; because the copper can be again recovered, when, as must be done of course, the scoriæ and litharge produced in the refining of the lead impregnated with copper, together with the metal imbibed by the ashes of the test, are again reduced to the reguline state; in which state they are conveyed again into the furnace with a copper-mat less roasted. The sulphur of this last again separates the copper from the reduced lead. But for the accomplishment of this condition it is necessary, that by small experiments, easy to be made, and in the manner indicated, it should be first ascertained, what degree of roasting of the copper-mat may be the most suitable to the operation. And with this view the portion to be subjected to the trial should be so managed, that an uniform degree of roasting may be kept up. According to my experiments in the small way, it seems, that copper-mat, which has been roasted the third time in the usual manner, and has once more passed by itself through the melting furnace, is apparently the best qualified for this purpose, if pure lead be made use of in divesting it of its silver. For though this last should happen to take up some of copper in the process, yet the copper may be recovered from it, by adding copper-mat of the second roasting to the litharge, with the first waste and metallic portion absorbed by the test at the time when these products of the refinery are afterwards reduced.

We see, therefore, that my method favours: (1) The expences of building and maintaining the house appropriated to the process of eliquation; (2) The wages expended on this account; (3) The additional charges for the fuel in the operations of eliquating, second eliquating



eliquating (darren)\* and melting the lead with the black-copper, in which it is contained ; (4) All the loss of lead, occasioned by its fusion with the copper at its eliquation, its refining, and the scratch-work with the brush in cleaning ; (5) Likewise the loss of lead poor in silver, which is obtained by eliquation from copper, and ought not to be enriched with silver in that degree, as may be done according to my method. And if it should be found adviseable to *refine* the *work-lead* directly in the smelting house, there would then (6) be saved the charges of transporting the black copper to the house, in which it is to be eliquated.

To this may be added, that by my method of proceeding the quality of the copper is improved, which is always the chief object of the smelting house. The copper obtained by eliquation is well known to contain lead, and this proportion of lead imparts to it a noxious property, diminishes its ductility, and even that of the brass, prepared from it. This contamination of the copper with lead cannot take place in my process ; because by that management no lead can combine with the copper of the copper-mat made use of, when the sulphur of this last has been sufficiently diminished in its quantity by the roasting.

## II.

*Observations on the Proofs of the Huttonian Theory of the Earth, adduced by Sir James Hall, Bart. By R. KIRWAN, ESQ. Communicated by the Author.*

(Concluded from page 102.)

### EXPERIMENT THE FIRST.

**G**RUNSTEN, a compound of felspar and hornblend intimately mixed with each other, was the subject of this experiment.

Its colour, black, or greenish black, intermixed with pale reddish brown ; both the felspar and hornblend imperfectly and confusedly crystalized in minute grains ; the fracture partly striated and partly foliated. Lustre moderate : its hardness 7, or almost 8. Gives an earthy smell when breathed upon, and frequently contains small specks of pyrites. Ibid. p. 7.

This substance he vitrified by a strong heat and subsequent rapid cooling, p. 9. A fragment of the glass thus produced being introduced under a narrow muffle, and heated to 21. in one minute became so soft as to yield readily to the pressure of an iron rod, but after a second minute it became quite hard, though the temperature had been stationary. The substance thus hardened underwent a thorough change, it lost its vitreous character, its fracture was like that of *porcelain* (that is even) and it was fusible only in a heat of 31. page 11.

\* *Gren's Principles of Chem.* §. 1490.

In another experiment, *ibid.* he found this change to take place even before the glass was in perfect fusion. For while both ends of a fragment of this glass were supported on rests of clay, it was found not to sink down between them until the heat was raised to 30. In another experiment he found the consolidation, which he (improperly, as I think) calls crystallization, to take place even while the heat was gradually increased, and the substance still so viscid as to retain the original shape of the fragments.

In another experiment, where the glass was slowly cooled, its texture was found completely to resemble that of whinstone, the *fracture* was *rough, stony, and crystalline*, with a number of shining facettes interperfed through the mass, and a few crystals in the cavities produced by air bubbles, p. 8.

These experiments may be considered in two points of view; first, with respect to phenomena of consolidation in a heat either gradually increased *above*, or gradually diminished *below* the heat necessary to soften the vitreous substance, the loss of the vitreous character, and the stony appearance, assumed through slow refrigeration.

And, in the second place, we may examine how far the phenomena here observed tend to countenance the Huttonian theory either of the formation of granite, trap, or basalt, or other stony substances: in this respect only it concerns me to examine these experiments, yet I cannot forbear mentioning some few reflections on the first.

It has been observed by all those who have attended to the formation of common glass (and is, indeed, evident from the fumes that float over its surface) that from the instant it enters into fusion, it is in a constant state of decomposition, gradually becomes less fusible, and increases in density; the substances that thus escape are, in this case, the saline, as Bosc D'Antic has shewn, and Macquer also asserts. See 1 Bosc D'Antic, 10, and 242, 213. and hence the loss of weight which glass thus suffers, *ibid.* 220, and 4 Macquer 261. Macquer also observed, that glass kept too long in fusion loses its transparency, and becomes opaque, because the flux evaporates. And he observes, that glasses formed of argill, lime, and gypsum, are particularly subject to this accident. Lavoisier noticed the same phenomenon during the fusion of felspar even by oxygen air, namely, that the longer it was kept in fusion, the more infusible it became. Mem. Par. 1783, p. 577, which he imputed to the volatility of one of some or other of its ingredients. And he afterwards found occasion to extend the same remark to steatites, and also to a mixture of equal parts of quartz and calcareous spar. This increased infusibility of certain substances by a gradually increased or continued heat, is not, therefore, a new discovery having been already noticed: but Sir James Hall has considerably enlarged it, by shewing that the stones he operated upon had re-assumed their stony appearance, after having been in a vitreous state; this appearance, if I understand him rightly, they have assumed only in consequence of slow cooling, and not merely by a heat either stationary or gradually increased; consolidation only being the effect of such treatment.

This consolidation Sir James calls *crystallization*, a term which seems to me highly improper; for, according to every sense in which this term has ever been employed, whether

that



that operation was perfect or confused, it denotes at least an union of particles previously dispersed through a liquid medium, they must, therefore, be at liberty to move through this medium in order to coalesce, and re-unite to each other (if both they and the medium itself coalesce and consolidate), this action is called *coagulation*, as happens in what was called *ossa Helmontii*, and the jelly formed by the *liquor silicum*; but in Sir James's experiment we find the consolidation to take place in a fragment of glass, which still retained its solid state, and, consequently, the particles were not at liberty to move towards each other. This consolidation must, therefore, evidently have arisen from some internal change in the constitution of the glasses in which it was observed. What these changes may have been I shall now examine.

In the first place, it is highly probable that flint, argil, and lime, and slightly oxygenated calx of iron, whatever be their affinity to each other when duly proportioned, require, like all solids, to absorb in their passage to a liquid state a certain portion of latent heat; but when in fusion, and the particles of each chemically united, they require a higher degree of heat to keep them in fusion; their elective affinities promoting fusion before the union, and impeding it after the union is formed; it is thus that iron and platina, metals separately highly infusible, contribute to each others fusion; but when fused, become still more infusible, as appears by Rinni. §. 135. Sulphur and lead are separately and easily fusible, but when united, their fusion becomes much more difficult. Again, Dr. Kennedy has discovered that all these whins contain 10 per cent. soda; and Vauquelin has lately discovered tartarin in felspar: in the high heats to which these stones are exposed in order to vitrify them, may it not be supposed that these salts are, in some measure, volatilized, and the compounds thus rendered less fusible? Though in an high heat rapidly produced, they may still be fusible as a smaller proportion of soda will in that circumstance suffice to that effect.

The next circumstance to be accounted for, is the *saxification* or *stony* appearance assumed by the vitrified stones when slowly cooled, by far the most curious fact, for which we are indebted to the ingenuity of Sir James. To account for this change, it is proper to remark; that though whins are said to be vitrified in a high degree of heat, yet this is not rigorously true, for in that case they should afford a transparent glass, whose fracture would be perfectly polished with a strong lustre, as we see that of common glass, whereas, in truth, they melt only into an *enamel*, nearly approaching to the perfect vitreous state: even the bottles made of them are nothing more; and hence their superior hardness. Their ingredients, therefore, are not uniformly diffused through their whole mass, but lie in the same order and position as before fusion, and in effect they contain much more flint than can be completely vitrified by the small proportion of lime and argill that enter into their composition, even though assisted by the soda; and in the next place we must notice, that the affinity of soda to flint diminishes in the same ratio as the heat diminishes, and, consequently, they separate, if the heat be not so suddenly diminished as to impede all motion. This is evident by what happens to common glass when slowly cooled down to the tem-

perature of the atmosphere, as came to pass in the Glass House at Leith, and conformably to this instance the faxification might take place even after a perfect vitrefaction;—that there are unions grounded on chymical affinity, which take place to a certain degree only at certain temperatures, and are in great measure loosened at a lower temperature, appears in the common instance of the solution of most salts in water, spirit of wine, or other menstruums, greater in a high degree of heat than at a lower, and has also been noticed in the fusion of gold in a mass of silver, for if the silver be very gradually cooled, the gold will separate from it, as Homberg observed, Mem. Par. 1713.

Now the affinity of filex to the alkali being loosened by a slight diminution of heat, the affinity of argill to the filex to which it united only as to a *compound* in the given temperature, is also necessarily loosened; that in the dry way argill unites to filex in temperatures below  $150^{\circ}$ , only in consequence of the previous union of the filex to the alkali, is clearly deduced from this fact, that if the alkali be absent, the union will not take place in temperatures below  $150$ , whereas it takes place by Sir James's own experiment, at temperatures below  $100$ , when the alkali is present; for he found the whins fusible at  $55^{\circ}$ . It is true the whins contain lime also, but though the presence of a certain proportion of lime contributes materially to the fusibility of filex and argill, yet it would be ineffectual in degrees of heat below  $120^{\circ}$ , if an alkali were not present to assist it, as I know by experience.

The presence of argill contributes also to the diminution of the affinity of the alkali to the siliceous ingredients, as the alkali seems to have nearly as strong an affinity (some think stronger) to argill as to filex; hence it is, that all analysts since Bergman's time employ an alkali to loosen the intimate union of filex and argill in precious stones.

Those facts being duly considered, we shall not be surprized at seeing the close vitreous texture destroyed by the slow cooling of melted whins, (all of which contain the above ingredients) and succeeded by the looser texture of a mere stony substance. This is the only change that takes place, if we except the minute and indeterminate crystallizations that occur in the cavities formed by the expulsion of air, while the mass was as yet soft; for the facettes interspersed through the stone cannot be accounted crystals, but only the rudiments of crystallization. These are formed at the instant the affinity of the alkali is lessened, and the earths begin to assume their solid state. The alkali being as yet liquid, allows the earthy particles to move through it, and to form these incipient crystallizations.

We are now to examine how far the stony structure assumed through slow refrigeration, by stones previously fused, tends to afford any support to the Huttonian theory. In my opinion it affords none at all; the utmost effect it can produce in an unprejudiced mind, is to render the origin of whins ambiguous, by making them assume the appearance of a Neptunian origin, when in fact they owe it to fusion; but it is only an *appearance*, for natural whins are accompanied with circumstances, and contain substances which contradict that appearance, and prove it to be deceitful. Besides, these experiments have no relation whatsoever to granite, or calcareous masses, which form the bulk of the globe, and afford



not the slightest indication of their origin; whins, though they abound in Scotland, and some other countries, are, in comparifon of the former, but thinly fattered over the furface of the globe. Some refemblance betwixt them and lavas has been long noticed. I fhall now briefly mention a few of the difcriminating characters of the artificial and natural whins, which may in moft cafes prevent us from confounding them, or afcribing to them a common origin.

1. The *natural* whins, particularly Amygdaloides (vulgarly called Toadftones) frequently contain calcareous fpar and zeolyte; now as the former contains fixed air, and the latter a notable proportion of water, I hardly think Sir James, who profeffes not to agree with Dr. Hutton in all points, will allow thefe to have been vitrified or fufed.

2. The natural whins, according to Dr. Kennedy's ftatement, lofe 5 per cent. of water, and other volatile matter, when heated to rednefs. It is not faid, whether the artificial lofe any part of their weight by fuch treatment; but it is plain they would not, fince even the lavas of Catania and Piedmonte, though of ancient date, loft none, as Dr. Kennedy exprefsly notices, and has thus afforded an excellent criterion for diftinguifhing the long contefted origination of thofe fubftances.

3. As Sir James has neglected giving a *complete* account of the external characters of the natural whins, which were the fubject of his experiments, as alfo of the regenerated, or artificial whins derived from them; and as I have not myfelf feen them, it is difficult for me to compare them with each other, and would, indeed, be impoffible, if fome account of them had not been given by Mr. Pictet, in his valuable *Journal Britannique*, copied into the 5th Vol. of the new Rozier's Journal, p. 313. It is the refult of the examination both of the natural and artificial whins by the Society of Natural Hiftory at Geneva.

As to the grunftein, No. 1. they remark, that it betrays not the leaft mark of an igneous origin, but that the whins which Sir James produced from it, had every diftinctive character of a lava, and even of a porous lava.

The Bafalt (or rather Trapp) on which the Caftle of Edinburgh ftands, is of a compact ftructure; the artificial produced from it, Sir James tells us, fo greatly refembles it both in colour and texture, that it would be difficult, or perhaps impoffible, to diftinguifh them, *but for a few minute air bubbles, diftinguifhable in the artificial.* Neptunifts will, however, confider this as a leading character of diftinction. The mineralogifts of Geneva add, that the colour of the artificial is deeper, and its hardnefs greater, than that of the natural. If the fpecific gravity, and other characters of both were given, it is probable that other differences might be perceived. It is only in thefe characters that any difference can be expected, as the internal compofition muft be the fame in both.

Of the remaining artificial whins I can give no account, their external characters having been omitted; I cannot, however, pafs over the general inferences that Sir James deduces from his experiments, namely, that "the arguments againft the fubterraneous fufion of "whinfone, derived from its ftony character, feem now to be fully refuted," for not to repeat

repeat what has been already said, that many of them contain substances whose existence is incompatible with that hypothesis, I must farther add, that the upright state in which many of them exist, for instance the basaltic pillars of Staffa, and of the Giant's Causeway, and of many other countries, the basis they rest on, sometimes granite, sometimes gneiss, sometimes coal or limestone, and the total absence of all signs of the operation of fire, forbid us to entertain any doubt of their production in the moist way. Nay, the College of Dublin now possesses fragments of basaltic pillars, in which marine shells are imbedded; if such evidence can be resisted, it is in vain to seek for greater.

Sir James thinks the cause of the fluidity of lavas, which I formerly suggested, as strange and inconceivable as that of Citizen Dolomieu. Not having had the happiness of viewing those stupendous torrents, I founded my opinion on the accounts given by the most accurate observers, and particularly of C. Dolomieu, who beheld, and carefully examined, every circumstance relating to them for many years. This great observer has not thought my opinion so inconceivable, for he has since embraced it. "From the manner" (says he) "in which lavas flow, it cannot be doubted, but they carry with them a substance capable of maintaining their heat and fluidity, and contain a substance which burns in contact with the atmosphere until it is consumed. This substance, of which sulphur is at least one of the principal ingredients, if it be not the only one, bears a strong resemblance in its constitution to phosphorus, being capable of two sorts of combustion. This combustion seems capable of maintaining fluidity in a bed of lava, &c." *1 New Rozier's Journ. p. 119 & 120.*

Sir James says, I have supposed substances that have left no trace of their existence. Other observers, however, discovered these traces, as Dolomieu and Fabroni, in the passages I have already quoted. *Mineralogy, Vol. I. p. 397, and 1 New Roz. p. 120, 121.* It is not to be expected that volatile substances, such as sulphur and petrol, should long remain. However, I acknowledge that the cause of the stony appearance which lavas after cooling exhibit, discovered by Sir James, appears to me at present by far the most probable; and that in this respect his discovery is of great importance to geology. But I persist in thinking, his experiments afford no confirmation of the high degrees of heat attributed to volcanos, and still less to the many hypotheses gratuitously heaped on each other by Dr. Hutton, or to the volcanic origin of whins or traps, for the reasons already assigned.



## III.

*On the Genuineness and Purity of Drugs and Medicines. By Mr. FRED. ACCUM.*

(Continued from page 36.)

## METALS AND METALLIC SUBSTANCES.

*Mercury. Quicksilver.—Ph. L.*

THE property which mercury possesses of readily uniting with most of the metals, without suffering any material change in its obvious properties, induces fraudulent dealers to adulterate this article with lead, tin, &c. If these metals are present in any considerable quantity, the mercury will have a dull aspect, and will not run freely into round globules, but when gently moved, it forms vermicular striæ, or tears. When agitated in a bottle, it soon becomes black, the lead and tin becoming oxidated, and may thus be separated, as has been noticed in the Philosophical Journal I. But as these metals are so easily detected, and can only be added in a comparatively small quantity, the dealers in this article have recourse to other practices. It is a fact, that bismuth and zinc remarkably favor the union of lead with this metallic fluid, and that even tin may then be added in a considerable quantity. As it is impossible to free mercury from these metals completely by a mere agitation, recourse must be had to distillation; and even then the mercury cannot be obtained perfectly pure, unless the process be very carefully managed.

Mercury free from any admixture should be totally volatile, when gently heated to the boiling point. In order to detect the presence of lead, one part must be boiled for a few minutes with twelve parts of electric acid. The fluid is then to be decanted, and examined by means of fulphuric acid. If the smaller quantity of lead be present, a few drops of this acid will render the fluid turbid, and a white precipitate will be separated. One hundred grains of this precipitate, well washed and dried, contains 72 parts of metal, and 28 of acid; the quantity of lead contained in a given quantity of mercury, may thus be accurately ascertained. But the minutest quantity of lead in the greatest quantity of mercury, can only be detected by dissolving the mercury in nitric acid, and then mixing this solution with water saturated with sulphurated hydrogenous gas. Dark brown clouds will immediately appear, and a precipitate of the same colour will be deposited, if the fluid is suffered to rest undisturbed for a day or two, one part of lead may thus be separated from 15260 parts of mercury. The admixture of bismuth is detected in a similar manner, by pouring the nitrous solution into distilled water; a white precipitate will appear if this metal be present. Tin is manifested in the usual manner, by a weak nitro-muriatic solution of gold; and zinc, like the rest of the metals, may be separated by exposing the mercury to heat.

*Sweet*

*Sweet Muriate of Mercury. Calomel.—Ph. L.*

Calomel ought to be perfectly saturated with mercury. Compleat saturation can only be known, by boiling for a few minutes one part of calomel, and  $\frac{1}{32}$  part of muriate of ammoniac in 10 parts of distilled water. The fluid must then be filtrated and examined by means of carbonate of pot-ash. If the calomel is well prepared, no change will take place on the addition of this re-agent; but if the preparation is imperfect, a precipitate will ensue, 47 parts of which indicate 48 of muriatic acid. 114 parts of calomel perfectly saturated with mercury, contain 97 of metal, and 17 of acid. It should be perfectly inodorous and tasteless, and when rubbed in a stone mortar with ammoniac becomes intensely black.

*Red sulphurated Oxide of Mercury. Red sulphurated Quicksilver.—Ph. L.*

The cinnabar of the shops is generally adulterated with red oxide of lead, in order to make it dry sooner when used for oil painting. To discover this reprehensible focus, one part of cinnabar is to be digested by heat with four of acetic acid. If lead is present, the acid will acquire a sweetish taste, and by letting fall into it a few drops of sulphuric acid, sulphate of lead will instantly be separated. Genuine red sulphurated oxide of mercury should be entirely volatile in the fire, and consequently leave nothing behind it after evaporation. A compleat decomposition of its integrant parts should be effected, by boiling one part of it in twelve of nitro-muriatic acid, (composed of three parts of nitric, and one of muriatic acid) and then treating the residue in a similar manner, with a sufficient quantity of soda, or pot-ash, forced from carbonic acid. Cinnabar is likewise found adulterated with a mixture of chalk and dragon's blood, and cinnabar thus contaminated, is of an exceedingly fine crimson colour. If it contains carbonate of lime, an effervescence will ensue on muriatic acid being added, and the earth will be taken up by the acid; on adding distilled water, and separating the fluid, chrystals of selenite will be obtained, on adding a little sulphuric acid. The colouring matter may be compleatly extracted by digesting the residue in ardent spirit, and subsequent evaporation. 100 parts of English cinnabar contain generally 20 parts of sulphur, and 80 of mercury. Its specific gravity is then 1000.

*Black Oxide of Mercury. Quicksilver with Sulphur.—Ph. L.*

This mixture of quicksilver and sulphur, ought to be at least so far completed, that no globules of mercury should be perceptible through a good microscope. Its colour is then of a beautiful black; it is impalpable to the touch, and totally volatile by heat. It is found sophisticated with ivory black. If this be the case, it will leave some ashes behind when laid on a red hot iron, or on ignited coals. If the mixture of the mercury with the sulphur has been well performed, the powder does not give a white colour to gold, when rubbed on it for any considerable time, nor can a separation be easily effected by saturating it with a thick mucilage, &c. However, black oxide of mercury, which stands these tests, will not be found in commerce, nor can it reasonably be expected.

*Nitrated*



*Nitrated Oxide of Mercury. Red Nitrated Quicksilver.—Ph. L.*

Most of the red nitrated oxide of mercury met with in the shops is adulterated with red oxide of lead. This fraud may be discovered in the same manner, as in the red sulphurated oxide of mercury. Genuine red nitrated oxide of mercury is totally volatile on being exposed to a red-heat, and soluble in nitric acid, without effervescence, but with a development of heat. Sulphuric acid converts it into yellow oxide of mercury (Turpeth mineral); muriatic acid forms corrosive muriate, and when completely saturated, sweet muriate of mercury, or calomel\*.

*White Oxide of Mercury. White Calx of Quicksilver.—Ph. L.*

Instead of pure white oxide of mercury, we frequently meet with a mixture of white oxide of mercury, and white oxide of lead, to which, not seldom, a considerable quantity of chalk is intimately mixed. White oxide of lead is discovered by digesting one part of the oxide with four of acetic acid, decanting the fluid, and adding to it a small quantity of sulphuret of ammoniac, or water impregnated with sulphurated hydrogen. An almost black precipitate will be formed, which on the addition of sulphuric acid will not be redissolved. The presence of calcareous earth may be investigated as directed before.

Genuine white oxide of mercury is of a snowy whiteness, tasteless, and inodorous; insoluble in acids, ponderous; does not become black when rubbed with fresh prepared lime-water, is totally volatilised by heat, and when accurately prepared, may thus be converted into calomel by mere sublimation.

*Yellow Oxide of Mercury. Vitriolated Quicksilver.—Ph. L.*

Is seldom found adulterated. But from a careless and slovenly management during the process for obtaining it, it often contains a considerable quantity of free sulphuric acid. It then has a perceptible acid taste, and disturbs the solution of muriate of barytes. It should be totally volatile by heat, and possess most of the characteristics of the preceding mercurial preparations.

*Sweet Muriate of Mercury obtained by Precipitation. Mild Muriated Quicksilver.—Ph. L.*

This preparation being precisely the same as calomel, ought therefore to possess all its peculiar properties. But as it is specifically lighter than calomel, it is found adulterated with starch, or with white oxide of bismuth. The first fraud may be detected by exposing it in a close vessel to a red heat; as by this means the calomel will sublime, and leave a

\* This product, if obtained according to the rules of the Royal College of Physicians, must be contaminated with a certain quantity of corrosive sublimate, on account of the prescribed quantity of muriatic acid to be used. Did the guardians of health wish to unite a certain quantity of this deadly poison with the red oxide? or did they credit the notion, that the product would have a more sparkling appearance, which in general is looked upon as the characteristic of excellency, &c.?

black coal behind, and the latter sophistication is detected by exposing it to heat in an open vessel, and reducing the residue by means of a little charcoal to the metallic state, for further investigation.

*Red Oxide of Mercury. Calcined Quicksilver.—Ph. L.*

Is seldom found adulterated. If well prepared it is totally volatile by fire. It is soluble in sulphuric, nitric, and muriatic acid, but the acetic, oxalic, malic, and tartareous acids have no effect upon it.

*Corrosive Muriate of Mercury. Muriated Quicksilver.—Ph. L.*

It has been said that this product has been found adulterated with arsenic. Though this may probably seldom be the case, yet when it is suspected, one part of the corrosive sublimate may be dissolved in 24 parts of distilled water, and precipitated again by carefully adding a solution of carbonate of pot-ash. The white precipitate thus afforded, is then separated from the supernatant fluid. This fluid thus freed from all the mercury it contained, is then to be tried by means of the ammoniate of copper. A transparent blue liquid will be formed, if the corrosive muriate of mercury was free from arsenic; but a yellowish green precipitate will appear if arsenic was present. This precipitate collected, dried, and laid on ignited coals, will soon diffuse the garlic smell peculiar to that metal. Two hundred and sixty-seven parts of this precipitate, contain one hundred and sixty-two parts of copper, and one hundred and sixty-five of arsenic.

Sixty-nine parts of corrosive muriate of mercury contain, if well prepared, generally 50 parts of mercury, 17 of solid muriatic acid, and two of water of crystallization. One part is then soluble in sixteen parts of cold water at 50°, in 3½ of boiling water, in 2½ of cold ardent spirit, or in a little more than its own weight of boiling ardent spirit.

*Iron. Iron.—Ph. L.*

The iron filings of the shops, which in general are procured from the gun-smiths, and other artificers, are never free from copper or brass. If some of the filings be dissolved in pure muriatic acid, and a polished iron be immersed into this saturated, and concentrated solution, the part of the metal in contact with the fluid, will soon become coppered, if the quantity of this metal was considerable. A smaller quantity of copper is manifested by digesting the iron filings, in water impregnated with ammoniac, which will acquire a blueish hue, if the filings contain copper. But the best way to detect the minutest quantity of copper or brass is, to dissolve one part of the suspected iron in three of nitric acid, and to decompose this solution by the addition of carbonate of pot-ash, or ammoniac freed from carbonic acid. In the first case, a greenish precipitate, and in the latter, a blue solution will be obtained, particularly if the ammoniac is added in abundance.

The practice of purifying iron filings usually made use of in the shops, by means of a magnet, is not so perfect as is generally expected. When other metals have been united with



with the iron, as is the case in various folders, the mere iron filings of the compound can not be separated in this manner. And so likewise brass, copper, and zinc filings, which adhere to those of the iron, are likewise forced to obey the magnet, and render the operation so far ineffectual.

*Sulphate of Iron. Vitriolated Iron.—Ph. L.*

Should consist entirely of iron united to sulphuric acid. The vitriolated iron of the shops frequently contains copper, on account of its being in general prepared, by merely re-dissolving the common green vitriol of commerce, which abounds with copper. The best way of detecting the presence of copper, consists in dissolving one part of the sulphate of iron, in three of distilled water, precipitating this solution with carbonate of pot-ash, and then letting fall into it a little ammoniac. If the smallest quantity of copper is present, the fluid will acquire a sapphire blue colour, but no such effect will take place if the salt be free from copper.

The practice of obtaining this salt pure, by dissolving the common green vitriol, and subsequent boiling with iron filings, or crystallization in an iron vessel, is not so satisfactory as could be wished. Sulphate of zinc, which is frequently found in the common sulphate of iron of commerce, does not become separated in this manner. A direct combination is requisite for obtaining this salt perfectly pure.

FRED. ACCUM.

No. 3, Compton Street, Soho.

IV.

*Extracts of Letters from H. GOODWYN, Esq. on the Unities of Weight and Measure best adapted to the British Empire; on the new Measures of France; with a Description of an Engine for raising Water.*

I APPREHEND it to be an indisputable point that a corresponding unity of weight and measure is truly desirable in this kingdom; and that an additional benefit, beyond what the French nation possess from their new metrical system, would arise, if that unity corresponded also with the general method in which the comparative specific gravities of bodies are expressed with us, namely, by an unit, or by one thousandth part. Now, Sir, it appears to me that we are not only in possession, but in the constant use of one, both for weight and measure, as invariable as that now established in France. I allude to the foot measure and avoirdupoise, or (if I may be allowed the more appropriate expression) decade ounce weight. When I add, what I am persuaded, by your valuable publications, you well know, that the decade ounce weight, of pure rain, or distilled, water at 60° of heat, is generally allowed to be equal in bulk to the  $\frac{1}{1000}$ th part of the cubic foot; and also ob-

serve that by adding only 44.3511 parts out of 10000; or about  $\frac{1}{225}$ th part, to the content of our present *Winchester measure*, that it would then contain exactly 10 cubic feet. These transitions from the present to such an improved system, would be "a consummation devoutly to be wished," and most ardently do I wish it to be legalized.

A standard measure for the purposes of trade, in particular, as well as for others, that would uniformly give an accurate result, and could be easily made, examined and ascertained, by common mechanics, which neither our present liquid or dry measures evidently can, would surely be an acquisition of great value. Such an one, I humbly presume, would be the following:—A square pyramid, whose perpendicular height is exactly thrice the length of the side of the base: for such an one, and every section of it, made by a plane parallel to its base, would, in the first instance possess, and in every subdivision, retain these remarkable properties\*.

1st. Similar comparative dimensions to those above given, for the original pyramid, *i. e.* every smaller pyramid, formed by the above-mentioned parallel section, would have its perpendicular height thrice the length of the side of its base, and

2dly. The length of the side of each base will always indicate, or equal the cube root of the solid content of the pyramid *e. g.* If the length of the side of the base be 3. the solid content will be the cube of 3. viz.  $3 \times 3 \times 3 = 27$ .

Mr. Locke has somewhere in his writings stated it as wrong for one man to pull down the superstructure of another without building or erecting a better in its stead. I some time past took the liberty to point out to you an error in the comparative tables of English and French measures in your Journal; and upon Mr. Locke's principle I feel it just to give you my calculation of the general heads of comparative tables between such a system as is advanced in the former part of this paper, and that I apprehend to be now established in France, premising, that I take the length of the *mètre* from your Journal, vol. III. page

283, at 

pieds.	pou.	lignes.
3	0	11.296,

 and I take the comparative length of the English, with the French foot, from data in the *Connoissance des Temps* for 1795, and from the *Philosophical Transactions* for 1768, page 326.

By these references it will appear that the French foot is to the English

as 1. : 1.065752004, &c.

Consequently,

French foot.

English decades, or 10ths of an English foot.

1.

=

10.65752004

\* I have been many years in the habit of using a pyramid measure to examine corn; and am perfectly convinced that such a one will indicate a far more accurate result than can arise from the manner in which corn is measured by the bushel.—G.



Comparative Tables, English with French.

LONG MEASURE.

Long decade.		Mètre.		Mètre.		Long decades.
1	=	0.03047983	ferè	1	=	{ 32.808583358, &c. or inches 39.3703

SQUARE MEASURE.

Square decades.		Ares.		Ares.		Square decades.
1	=	0.000092902	ferè	1	=	{ 107640.3142 or sq. inches 155002.052448.

CUBE MEASURE.

Cube decades.		Litres.		Litre.		Cube decades.
1	=	0.02831637	ferè	1	=	{ 35.3152622, &c. or cubic inches 61.0247727.

WEIGHTS.

Avoird. or decade oz.		Grammes.		Gramme.		Decade oz.
1	=	28.31637	ferè	1	=	{ 0.03531526, &c. or grains or 15.45042625.
Long, Square, or Cube.	{	decades are reduced to		Long, Square, or Cube.	{	English inches by multiplying by
						{ 1.2 1.44 1.728

And decade ounces are reduced to grains,

containing, { 7000.  
or  
5760. } to the lb. { Avoird. } by  
Troy }

multiplying the ounce by 437.5 = the number of grains in an avoirdupoise ounce.

East Smithfield, April 25, 1800.

Description of an Engine for raising Water.

THE hydrostatic machine here described was invented by me some years ago. It has been seen but by few: Mr. Watt, of Birmingham, Mr. Rennie, the civil engineer, Mr. More, the late secretary to the Arts and Sciences, Mr. George Adams, late of Fleet-street, who has made handsome mention of it in his Lectures. I have no doubt but you can readily point out many uses to which it may be applicable.

May 17, 1800.

H. G.  
A Section

*A Section and Description of a Machine that will raise a Body of Water to any Height, not exceeding the Height of a Column that will counterbalance the Pressure of the Atmosphere (say 30 Feet) by the Descent of Part of the same Body of Water, through a somewhat greater Height, and aided by the Pressure of the Atmosphere.*

- Let A      Plate VIII. figure 2, be a sphere of copper, or other metal, about 1 foot 6 inches in diameter.
- B      Another sphere about 2 feet 6 inches in diameter.
- C      A reservoir of water kept constantly supplied.
- D      A glass cap about 6 inches long, fixed on the top of A, for the purpose of seeing when the water begins to fill and has filled A.
- E      The upper reservoir into which A is to be emptied.
- 1, 1,      A pipe, about 0.5 inches in diameter, fixed into the top of B, and rising upwards to within about an inch of the top of D.
- 2      A pipe of the same diameter, a few feet longer than 1, 1, fixed into the bottom of B, and extending downwards in a perpendicular or inclined direction.
- 3      A pipe of one inch and a half in diameter, fixed to bottom of A, and extending upwards through it, to within about two inches of the top of D. *Vide fig. 3.*
- 4, 4,      A pipe about half an inch diameter fixed into the top of B, and extending upwards, through, and fixed to bottom of C.
- 5      A pipe of same diameter fixed to top of B, and terminating in and fixed to bottom of C.
- 6      A pipe of same diameter fixed into bottom of A.
- 7      A trumpet mouth pipe fixed to bottom of pipe 3, and extending downwards, to within about an inch of bottom of reservoir C.

*a, b, c, & d* Are cocks fixed to the pipes.

The spheres, pipes, cocks, and joints, must all be air tight.

In order to raise water from reservoir C into reservoir E, all the cocks being turned off, proceed thus: turn on the cocks *b* and *c*, in order to fill B, and when B is full, turn off the cocks *b* and *c*, and turn on the cock *d*. The water will then begin to run from the sphere B, and will, by means of its communication through the pipe 1, 1, with the upper sphere A, rarify the air therein, and the atmosphere at the same time pressing on the water in C, will cause it to rise through the trumpet mouth at 7, of pipe 3, and by falling over the top of it at D to fill the sphere A. When A is full, which may be seen through the glass cap D, turn off *d*, and turn on *a, b*, and *c*, by which means A will empty into the reservoir E, and B will be replenished for another operation. Then turn off the cocks *a, b*, and *c*, and turn on the cock *d*, and repeat the operation of raising the water into A. But if it be required



quired to raise *one body* of water from reservoir C into reservoir E, by means of the descent of *another body* of water through B, a communication must be made into B, independant of the pipe *g* and cock *c*, viz. through a pipe, cock, and funnel (or reservoir) represented by the dotted lines, communicating with B, near the pipe and cock *g* and *c*, and then they are to be used in lieu of pipe *g* and cock *c*.

V.

*A Memoir, in which the Question is examined, whether Azote be a simple or compound Body?*  
By CHRISTOPHER GIRTANNER, Doctor of Physic at Gottingen.

(Concluded from page 140.)

I PROPOSE, in a particular work upon azote, to give a detail of the numerous experiments which I had made to discover its nature: I shall confine myself at present to give a general outline of these experiments, as well as of the inductions which I have thought might be drawn from them.

I think myself intitled, therefore, to affirm without self deception, that azote gas is obtained.

1. When water is boiled in an earthen retort not glazed within, and the vapour is made to pass through a tube of glass, or other material;

2. When water is boiled in a retort of glass containing clay or alumine, and the vapour is made to pass through a tube of glass, or other material;

3. When water is boiled alone in a retort of glass, and the vapour is made to pass through a tube of pipe clay;

4. When water is boiled alone in a glass retort, and the vapour is made to pass through a glass tube containing clay or alumine;

5. When an earthen tube is filled with water, and this tube is included in a larger tube of glass with sand between them, and this tube of glass in tube of iron also with sand between them, and the whole is exposed to ignition \*;

6. When water is boiled in a glass retort containing lime, and the vapour is passed through a tube of glass or other material;

7. When water is boiled in a glass retort containing pounded quartz, or flint, and the vapour is passed through a tube of glass, or other material;

8. Experiment 1 succeeds equally when the earthen retort is coated externally with a metallic glaze:

\* This experiment, which I have not repeated, is given by Priestley, in his experiments and observations on different kinds of air. Birmingham, 1790, vol. ii. page 418.

9. Experiment 3 succeeds equally when the earthen tube is coated *externally* with a metallic glaze :

10. Experiment 4 succeeds equally when the tube is filled with lime, or pounded glass.

The vapours of water only without azote gas are obtained,

1. When water is boiled in a glass retort, and the steam is passed through tubes of glass or porcelain ;

2. When water is boiled in an earthen retort, internally coated with a metallic glaze, and the vapours are passed through tubes of glass or porcelain ;

3. When water is boiled in a glass retort filled with pounded glass, and the vapours are passed through the tubes of glass or porcelain :

### GENERAL REMARKS.

In order to obtain azote gas in considerable quantity, the water must be gently evaporated by a very moderate heat, and care must be taken not to raise it.

It may be observed, that in all these experiments without exception, that as soon as the last drops of water is evaporated, the azote gas ceases to be produced, notwithstanding the continuance of the heat.

### RESULTS OF THE EXPERIMENTS.

Such are the facts, and the simple enumeration of experiments, independent of all system, explanation, and theory. And it follows,

1. That Messrs. Wiegleb and Goettling were right in affirming, that water is changed into azote gas by the action of heat ;

2. That they were wrong in maintaining that this change takes place always, and in all circumstances, provided the vapours of water be in contact with ignited bodies ;

3. That Mr. Wiegleb has not proved what he advanced, namely, that the change of water into gas is owing to caloric, and that water is the basis of every gas whatever.

4. That the Dutch chemists were wrong in advancing, that the change of water into azote gas never happens in any case, and that the gas obtained is owing to the atmospheric air having passed through the retorts and the tubes ;

5. That there are circumstances in which water is changed into azote gas, and others in which it is not, and that it is easy to reconcile the two opposite parties.

The change of water into azote gas by the action of caloric and the earths being affirmed, it remains for us to discover the solution of this problem.

I have observed, as well as Ingenhoufz, Von Homboldt, and Van Mons, that the earths when moistened, have the property of absorbing the oxygen of the atmosphere at the ordinary temperature. I have also observed what Von Homboldt has not remarked, that they absorb oxygen in less time, and greater quantity, when they are heated. I found by other experiments, that the earths also take oxygen from water, but that it is necessary for this purpose, that the temperature should be more elevated than that of the atmosphere.

Clay,



Clay, loamy earth, or alumine, become charged with oxygen with the most avidity, and at a temperature far below that of boiling water. Lime requires a more elevated temperature, and does not absorb so much oxygen. Silix requires to be ignited before it will absorb oxygen, but then it seizes it rapidly enough. Baked clay also unites with oxygen, but requires a still more elevated temperature. Glazed clay does not absorb oxygen, because the glaze being a metallic glass, has no action upon that substance.

Having made these observations, I did not find much difficulty in explaining the phenomena of the change of water into azote gas: Recollecting the hypothesis which was thrown out by Mr. Mayer some years ago, on the nature of azote gas, which he supposes to be composed of oxygen and hydrogen, or that it is water changed into gas\*, I adopted similar ideas respecting the nature of this gas. Mr. Mayer having given his hypothesis merely as a conjecture, unsupported by any chemical experiment whatever, I proposed to supply what was wanting in the establishment of his conclusion.

Admitting this hypothesis, and reflecting on the singular property which the earths possess of absorbing oxygen from water, I explained the before related experiments without difficulty. They are the consequence of a double affinity. The oxygen of the water partly unites with the earth, and converts it into an earthy oxide; the rest of the oxygen uniting with the hydrogen combines with the caloric, and forms azote gas; whence it follows, that *azote is water deprived of part of its oxygen*.

Several experiments present themselves in support of this assertion. I shall confine myself to mentioning some of them, and shall speak of the others in a second memoir, when I shall have repeated them.

1. Let tubes of porcelain be procured and tried, by passing the vapour of boiling water through them, which will be condensed at the other extremity without the smallest particle of gas, excepting the air which is contained in the retort. After having in this manner ascertained that the water is not changed into gas in these tubes, in order to obviate the objections of those who imagine, that the external air passes through these tubes; let one of them be filled with tin filings and placed in a fire, taking care to keep it up: after which the vapours of water are to be passed through.

The pneumatic apparatus will be found to contain azote gas mixed with oxygen gas. The tin is changed into oxide, which oxide passes under the pneumatic apparatus with the azote gas, and the vapours of the water.

This experiment, which was made by Mr. Van Hauch, is easily explained according to my theory. The tin takes only part of the oxygen from the water. The rest unites with hydrogen and forms azote.

2. When the vapours of water are passed over lead in the same manner as in the foregoing experiment, a similar result is obtained according to Mr. Van Hauch. At the commencement of the operation, the oxide of lead passes into the inverted receiver along

\* Gren's Journal der Physik, vol. v. page 321.

with the gas, and the watery vapours; after which the metal itself appears in the form of an extremely fine powder. The proportion of azote gas to that of the oxygen obtained is as 64 to 36.

3. When the vapours of water are passed in the same manner through a tube filled with antimony, a mixture of azote and oxygen gas is obtained, in the proportion of 89 parts of the former to 11 of the latter.

4. A porcelain tube was filled with the black oxide of manganese. The tube was exposed to a very strong fire for two hours, till the oxide had entirely ceased to afford oxygen gas. The vapours of water were then passed over this oxide thus deprived of the greater part of its oxygen. Another product of oxygen gas of considerable purity, was first obtained, and afterwards azote gas.

This experiment of Mr. Van Hauch is very instructive. The manganese first absorbed the hydrogen with avidity, and afterwards part of the oxygen of the water.

5. Through the same tube filled with the same manganese, as had served for experiment 4, the vapours of water were passed. Azote gas was obtained. The fire was kept up for near three hours, and the disengagement of azote continued as long as the vapours were passed through. It ceased when these vapours were stopped, but the disengagement of azote began again as soon as the water was again made to boil, and the vapours began to pass over the manganese. This experiment was repeated for six following days during three hours each day, and always with the same success. When the operation was ended, the manganese was found adhering to the porcelain, so that it could not be separated.

6. Dr. Pearson, in his analysis of water by the electric spark always obtained azote gas, besides the two gases which compose water.

7. The same Dr. Pearson, by burning a mixture of oxygen and hydrogen gases in a tube hermetically closed, obtained water and azote gas.

8. Dr. Priestley observed, that oxygen gas, which remains for a time in contact with the purest distilled water, becomes partly changed into azote. I verified this observation, and found as much as 0,1 of azote gas in the oxygen gas.

9. A mixture of hydrogen gas and nitrous gas, which remains for a time in contact with water, no longer burns, but is found to be changed into atmospheric air. I explain this fact by supposing, that the hydrogen has combined with part of the oxygen of the nitrous gas, or of the water, and has become changed into azote gas. This experiment, however, which was made by Mr. Link, did not succeed with me.

10. Dr. Priestley found that hydrogen gas, which he had kept for a long time in contact with water, was entirely changed into azote gas. This experiment does not always succeed. It succeeded four times with Priestley. It is no doubt necessary that the water should contain oxygen gas.

11. By burning together a mixture of eleven cubic inches of hydrogen gas, and one cubic inch of oxygen gas, azote gas is obtained. This experiment of Mr. Yelin did not succeed with me.

12. When



12. When the vapours of water are passed through a gun-barrel, which has already been used a number of times in this kind of experiment, and is intirely oxidized within, hydrogen gas is no longer obtained, but azote; the iron being no longer capable of combining with all the oxygen which the water presents to it. This experiment of Mr. Yelling has been confirmed to me by Mr. Mayer.

13. An experiment made by Mr. Lampadius, appears to me to afford very satisfactory proof that oxygen is contained in azote gas. He fused arsenic in the purest azote prepared by the combustion of phosphorus. The metal was sublimed, and after the experiment, he found that it was partly changed into oxide of arsenic.

14. When in the combustion of the two gases to produce water, the quantity of hydrogen is too great, a product of nitric acid is obtained.

15. The following experiment described by Scheele, appears to me to be a new proof that azote is nothing else but an oxide of hydrogen. Thus it is that this famous chemist expresses himself, in his treatise on Air and Fire, "I filled a bladder with air obtained from iron filings, dissolved in vitriolic acid, and I respired this air. After twenty inspirations I found myself obliged to stop. When I had recovered, I emptied my lungs as much as possible, and again respired the inflammable air. After ten inspirations I could proceed no farther. The air upon examination was no longer inflammable, and produced no cloud in lime water; in a word it was corrupted air" (azote gas).

*(To be continued.)*

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## VI.

*Further Remarks on the Preparation of Prussiate of Pot-ash—Method of purifying Caustic and carbonated Alcalis from Sulphate of Pot-ash. By Mr. WILLIAM HENRY.*

*Manchester, June 16, 1800.*

TO MR. NICHOLSON.

SIR,

IN your Journal for April last (p. 30), I communicated a new method of preparing the prussiate of pot-ash. The only objection I am aware of, that can be urged against this process, is the expenditure of a considerable quantity of prussiated barytes, a salt which it may not be in the power of every one to prepare. As the sole advantage, obtained by its employment, is the decomposition, by double affinity, of sulphate of pot-ash; it has since occurred to me, that a considerable saving of prussiated barytes would be gained, by first saturating the alkali, in the common way, with Prussian blue, and afterwards digesting the impure prussiate of pot-ash, thus obtained, with prussiate of barytes. To a solution of common prussiate of pot-ash, which immediately precipitated muriate of barytes, I added,

therefore, a small quantity of the barytic prussiate, and applied a gentle heat for a few minutes. The solution, when filtered, no longer precipitated muriate of barytes (except after some time in the manner I have pointed out, p. 32.) The purer the alkali, employed in making the prussiate of pot-ash, the less, it is obvious, will be the proportion of prussiated barytes required.

I have lately contrived a method of preparing pure and carbonated alkalis, which has many advantages over the common ones. Indeed the formation of a carbonated alkali, perfectly free from fulphate of pot-ash, is a problem of considerable difficulty, except by very expensive processes. Crystallization is incompetent to separate the whole of the fulphate. The lixivium of the ashes of tartar, and the alkali prepared from nitre, I have never found free from fulphate of pot-ash. By saturating the alkali with acetous acid, and then adding acetite of barytes, a pure acetite of alkali may be obtained, which, when decomposed by heat, affords a pure carbonate. But this mode is troublesome and costly. I recommend the following, as a cheap and easy process:

Render the alkali, whether vegetable, mineral, or volatile, perfectly caustic by quicklime; and to the clear solution add, by degrees, a warm solution of pure barytic earth, till the precipitation ceases. The barytes seizes the fulphuric acid; and leaves the alkali pure, which may, if required, be afterwards saturated with carbonic acid, in any of the common modes. During its restoration to a mild state, any barytes, that may remain in excess, is also precipitated. If the carbonic acid, employed for saturation, be obtained from carbonate of lime by fulphuric acid, the gas should be previously passed through a solution of carbonate of pot-ash, in order to separate any fulphuric acid, that may happen to be attached to it. A carbonate of alkali thus prepared, when saturated with acetous acid, gives no signs of fulphuric acid, on adding acetite of barytes.

Caustic alkali, however, purified in the above mode, still retains a small portion of lime; for, on examining the precipitate very attentively, I found no lime in its composition, which might perhaps have been expected in it, from some late experiments of M. M. Guyton, Kirwan, and Hatchett, on the affinity of earths in the humid way. If a caustic alkali be required, perfectly free from lime, as may sometimes happen in nice experiments, the alkali may be deprived of its carbonic acid, entirely from the beginning, by pure barytes.

I am, Sir,

Your obedient humble Servant,

WILLIAM HENRY.



## VII.

*On a New Fulminating Mercury.* By EDWARD HOWARD, Esq. F.R.S\*.

## SECTION I.

THE mercurial preparations which fulminate, when mixed with sulphur, and gradually exposed to a gentle heat, are well known to chemists: they were discovered, and have been fully described, by Mr. Bayen †.

MM. Brugnatelli and Van Mons have likewise produced fulminations by concussion, as well with nitrate of mercury and phosphorus, as with phosphorus and most other nitrates ‡. Cinnabar likewise is amongst the substances which, according to MM. Fourcroy and Vauquelin, detonate by concussion with oxymuriate of pot-ash §.

Mr. Ameilon had, according to Mr. Berthollet, observed, that the precipitate obtained from nitrate of mercury by oxalic acid, fuses with a hissing noise ||.

## SECTION II.

But mercury, and most if not all its oxides, may, by treatment with nitric acid and alcohol, be converted into a whitish crystallized powder, possessing all the inflammable properties of gunpowder, as well as many peculiar to itself.

I was led to this discovery, by a late assertion, that hydrogen is the basis of the muriatic acid: it induced me to attempt to combine different substances with hydrogen and oxygen. With this view, I mixed such substances with alcohol and nitric acid, as I thought might

\* Philof. Transf. 1800, p. 204.

† *Opuscles Chimiques de Bayen*, tom. i. p. 346, and note in p. 344.

‡ *Annales de Chimie*, tom. xxvii. p. 74 and 79. (or this Journal, II. 468.)

§ Ibid. tom. xxi. p. 238. (or this Journal, I. 168.)

|| This fact has been misrepresented, in the introduction to a work intitled *The Chemical Principles of the Metallic Arts*, by W. Richardson, Surgeon, F.A.S. Sc. (page lvii.) The author, speaking of the acid of sorrel, says, "Klaproth of Berlin precipitated a nitrous solution of mercury with acid of wood-sorrel, neutralized with vegetable alkali. The white precipitate, well washed and dried, produced a fulminating noise, not inferior to that of fulminating gold. Acid of sugar, perfectly neutralized by vegetable alkali, produced the same precipitate, which, on exposure to heat, exhibited the same fulminating power." I must confess, I have not been able to produce any such fulmination. Mr. Richardson has moreover given this supposed discovery to Mr. Klaproth; whereas, Mr. Berthollet, when quoting the fact to which I suppose Mr. Richardson intended to allude, observes, "Qu'on avoit déjà donné le nom d'argent fulminant au précipité du nitrate d'argent par l'acide oxalique, dans lequel M. Klaproth avoit découvert la propriété de fuser avec vivacité lorsqu'on l'expose à la chaleur. M. Ameilon avoit aussi, depuis longtemps, fait connoître que l'acide oxalique communiquoit cette propriété au mercure, quoique moins fortement qu'à l'argent; mais cet effet (he continues) est fort éloigné de celui qu'on désigne par la fulmination." *Annales de Chimie*, tom. i. p. 57.

(by predisposing affinity) favour, as well as attract, an acid combination, of the hydrogen of the one, and the oxygen of the other. The pure red oxide of mercury appeared not unfit for this purpose; it was therefore intermixed with alcohol, and upon both, nitric acid was affused. The acid did not act upon the alcohol so immediately as when these fluids are alone mixed together, but first gradually dissolved the oxide: however, after some minutes had elapsed, a smell of ether was perceptible, and a white dense smoke, much resembling that from the *liquor fumans* of Libavius, was emitted with ebullition. The mixture then drew down a dark-coloured precipitate, which by degrees became nearly white. This precipitate I separated by filtration: and, observing it to be crystallized in small acicular crystals, of a saline taste, and also finding a part of the mercury volatilized in the white fumes, I must acknowledge I was not altogether without hopes that muriatic acid had been formed, and united to the mercurial oxide. I therefore, for obvious reasons, poured sulphuric acid upon the dried crystalline mass, when a violent effervescence ensued, and, to my great astonishment, an explosion took place.

The singularity of this explosion induced me to repeat the process several times; and, finding that I always obtained the same kind of powder, I prepared a quantity of it, and was led to make the series of experiments which I shall have the honour to relate in this paper.

### SECTION III.

I first attempted to make the mercurial powder fulminate by concussion; and for that purpose laid about a grain of it upon a cold anvil, and struck it with a hammer, likewise cold: it detonated slightly, not being, as I suppose, struck with a flat blow; for, upon using 3 or 4 grains, a very stunning disagreeable noise was produced, and the faces both of the hammer and the anvil were much indented.

Half a grain or a grain, if quite dry, is as much as ought to be used on such an occasion.

The shock of an electrical battery, sent through 5 or 6 grains of the powder, produces a very similar effect: it seems indeed, that a strong electrical shock, generally acts on fulminating substances like the blow of a hammer. Messrs. Fourcroy and Vauquelin found this to be the case with all their mixtures of oxymuriate of potash\*.

To ascertain at what temperature the mercurial powder explodes, 2 or 3 grains of it were floated on oil, in a capsule of leaf tin; the bulb of a Fahrenheit's thermometer was made just to touch the surface of the oil, which was then gradually heated till the powder exploded, as the mercury of the thermometer reached the 368th degree.

### SECTION IV.

Desirous of comparing the strength of the mercurial compound with that of gunpowder, I made the following experiment, in the presence of my friend Mr. Abernethy.

\* *Annales de Chimie*, tom. xxi. p. 239.



Finding that the powder could be fired by flint and steel, without a disagreeable noise, a common gunpowder proof, capable of containing eleven grains of fine gunpowder, was filled with it, and fired in the usual way: the report was sharp, but not loud. The person who held the instrument in his hand felt no recoil; but the explosion laid open the upper part of the barrel, nearly from the touch-hole to the muzzle, and struck off the hand of the register, the surface of which was evenly indented, to the depth of 0,1 of an inch, as if it had received the impression of a punch.

The instrument used in this experiment being familiarly known, it is therefore scarcely necessary to describe it; suffice it to say, that it was of brass, mounted with a spring register, the moveable hand of which closed up the muzzle, to receive and graduate the violence of the explosion. The barrel was half an inch in caliber, and nearly half an inch thick, except where a spring of the lock impaired half its thickness.

#### SECTION V.

A gun belonging to Mr. Keir, an ingenious artist of Camden-town, was next charged with 17 grains of the mercurial powder, and a leaden bullet. A block of wood was placed at about eight yards from the muzzle, to receive the ball, and the gun was fired by a fuse. No recoil seemed to have taken place; as the barrel was not moved from its position, although it was in no ways confined. The report was feeble: the bullet, Mr. Keir conceived, from the impression made upon the wood, had been projected with about half the force it would have been by an ordinary charge, or 68 grains, of the best gunpowder. We therefore recharged the gun with 34 grains of the mercurial powder: and, as the great strength of the piece removed any apprehension of danger, Mr. Keir fired it from his shoulder, aiming at the same block of wood. The report was like the first in Section IV. sharp, but not louder than might have been expected from a charge of gunpowder. Fortunately, Mr. Keir was not hurt, but the gun was burst in an extraordinary manner. The breech was what is called a patent one, of the best forged iron, consisting of a chamber 0,4 of an inch thick all round, and 0,4 of an inch in caliber; it was torn open and flayed in many directions, and the gold touch-hole driven out. The barrel, into which the breech was screwed, was 0,5 of an inch thick; it was split by a single crack three inches long, but this did not appear to me to be the immediate effect of the explosion. I think the screw of the breech, being suddenly enlarged, acted as a wedge upon the barrel. The ball missed the block of wood, and struck against a wall, which had already been the receptacle of so many bullets, that we could not satisfy ourselves about the impression made by this last.

#### SECTION VI.

As it was pretty plain that no gun could confine a quantity of the mercurial powder sufficient to project a bullet, with a greater force than an ordinary charge of gunpowder, I determined to try its comparative strength in another way.

I procured

I procured two blocks of wood, very nearly of the same size and strength, and bored them with the same instrument to the same depth. The one was charged with half an ounce of the best Dartford gunpowder, and the other with half an ounce of the mercurial powder; both were alike buried in sand, and fired by a train communicating with the powders by a small touch-hole. The block containing the gunpowder was simply split into three pieces: that charged with the mercurial powder was burst in every direction, and the parts immediately contiguous to the powder were absolutely pounded, yet the whole hung together, whereas the block split by the gunpowder had its parts fairly separated. The sand surrounding the gunpowder was undoubtedly the most disturbed: in short, the mercurial powder appeared to have acted with the greatest energy, but only within certain limits.

## SECTION VII.

The effects of the mercurial powder, in the last experiments, made me believe that it might be confined, during its explosion, in the centre of a hollow glass globe. Having therefore provided such a vessel, 7 inches in diameter, and nearly half an inch thick, mounted with brass caps, and a stop cock, (see Plate VII.) I placed 10 grains of the mercurial powder on very thin paper, laid an iron wire 149th of an inch thick across the paper, through the midst of the powder, and, closing the paper, tied it fast at both extremities, with silk, to the wire. As the inclosed powder was now attached to the middle of the wire, each end of which was connected with the brass caps, the packet of powder became, by this disposition, fixed in the centre of the globe. Such a charge of an electrical battery was then sent along the wire, as a preliminary experiment\* had shewn me would, by making the wire red-hot, inflame the powder. The glass globe withstood the explosion, and of course retained whatever gases were generated: its interior was thinly coated with quicksilver in a very divided state. A bent glass tube was now screwed to the stop-cock of the brass cap, which being introduced under a glass jar standing in the mercurial bath, the stop-cock was opened. Three cubical inches of air rushed out, and a fourth was set at liberty when the apparatus was removed to the water-tub. The explosion being repeated, and the air all received over water, the quantity did not vary. To avoid an error from change of temperature, the glass globe was, both before and after the explosion, immersed in water of the same temperature. It appears therefore, that the ten grains of powder, produced four cubical inches only of air.

To continue the comparison between the mercurial powder and gunpowder, 10 grains of the best Dartford gunpowder were in a similar manner set fire to in the glass globe: it remained entire. The whole of the powder did not explode, for some complete grains were to be observed adhering to the interior surface of the glass. Little need be said of the nature of the gases generated during the combustion of gunpowder: they must have

\* With Mr. Cuthbertson's electrometer.



been, carbonic acid gas, sulphureous acid gas, nitrogen gas, and (according to Lavoisier \*) perhaps hydrogen gas. As to the quantity of these gases, it is obvious that it could not be ascertained; because the two first were, at least in part, speedily absorbed by the alkali of the nitre, left pure after the decomposition of its nitric acid.

## SECTION VIII.

From the experiments related in the 4th and 5th sections, in which the gunpowder proof and the gun were burst, it might be inferred, that the astonishing force of the mercurial powder is to be attributed to the rapidity of its combustion; and, a train of several inches in length being consumed in a single flash, it is evident that its combustion must be rapid. From the experiments of the 6th and 7th sections, it is sufficiently plain that this force is restrained to a narrow limit; both because the block of wood charged with the mercurial powder was more shattered than that charged with the gunpowder, whilst the sand surrounding it was least disturbed; and likewise because the glass globe withstood the explosion of 10 grains of the powder fixed in its centre: a charge I have twice found sufficient to destroy old pistol barrels, which were not injured by being fired when full of the best gunpowder. It also appears, from the last experiment, that 10 grains of the powder, produced by ignition four cubical inches only of air; and it is not to be supposed that the generation, however rapid, of four cubical inches of air, will alone account for the described force; neither can it be accounted for by the formation of a little water, which, as will hereafter be shewn, happens at the same moment: the quantity formed from 10 grains must be so trifling, that I cannot ascribe much force to the expansion of its vapour. The sudden vaporization of a part of the mercury, seems to me a principal cause of this immense yet limited force; because its limitation may then be explained, as it is well known that mercury easily parts with caloric, and requires a temperature of 600 degrees of Fahrenheit, to be maintained in the vaporous state. That the mercury is really converted into vapour, by ignition of the powder, may be inferred from the thin coat of divided quicksilver, which, after the explosion in the glass globe, covered its interior surface; and likewise from the quicksilver with which a tallow candle, or a piece of gold, may be evenly coated, by being held at a small distance from the inflamed powder. These facts certainly render it more than probable, although they do not demonstrate, that the mercury is volatilized; because it is not unlikely that many mercurial particles are mechanically impelled against the surface of the glass, the gold, and the tallow.

As to the force of dilated mercury, Mr. Baumé relates a remarkable instance of it, as follows.

“ Un alchimiste se présenta à Mr. Geoffroy, et l'assura qu'il avoit trouvé le moyen de fixer le mercure par une opération fort simple. Il fit construire six boîtes rondes en fer

\* See Lavoisier, *Traité élémentaire*, p. 527.

“ fort épais, qui entroient les unes dans les autres; la dernière étoit assujettie par deux  
 “ cercles de fer qui se croisoient en angles droits. On avoit mis quelques livres de mer-  
 “ cure dans la capacité de la première : on mit cet appareil dans un fourneau assez rempli  
 “ de charbon pour faire rougir à blanc les boîtes de fer ; mais, lorsque la chaleur eut  
 “ pénétré suffisamment le mercure, les boîtes creverent, avec une telle explosion qu’il se  
 “ fit un bruit épouvantable : des morceaux de boîtes furent lancés avec tant de rapidité,  
 “ qu’il y en eut qui passèrent au travers de deux planchers : d’autres firent fur la muraille  
 “ des effets femblables à ceux des éclats de bombes \*.”

(*To be continued.*)

### VIII.

*Ancient Account of Parhelia seen in Cumberland. Communicated by Mr. H. SARGEANT.*

To Mr. NICHOLSON.

S I R,

THE following is copied from a manuscript written in the beginning of the 17th century, and now in the library belonging to the Grammar School of St. Bees. As it relates to a phænomenon which is extremely rare, and in this instance probably not elsewhere recorded, I send it to you for insertion, if you think proper, in your valuable Journal, together with a copy of the figure (Pl. VIII. Fig. 4.) which accompanies it.

I am, Sir,

Your humble Servant,

*Whitehaven, 1st of June 1800.*

H. SARGEANT.

Upon the 8th day of May, anno 1597, in Copland, (a district of Cumberland) was seene aboute the sunne, beinge some two houres from settinge, and entringe into a thick cloude three parrhelii, or resemblances of the sunne, the brightest towards the north, the two other, one towards the south, and the other towards the easte, and in a thinne whitish cloude, hardly able to be discerned, appeared like a rayne-bowe, a pretye distance from each of the said parrhelii. These towards the north and south did seeme to be direct, streight, without any compassinge or bowinge in towards the endes, but stoode like direct lynes from the south towards the north. The one of them turned with his brightnesse towards the other, but the rayne-bowe, which was eastward from the caste parrhelios, did bowe or compasse eastward directly from the sunnwards, and from the parrhelios, which seemed contrary to the nature of all other rayne-bowes which ever I sawe.

N. B. The writer is supposed to be the Rev. Mr. Copeland, a graduate of Oxford, about the year 1600.

\* *Chymie expérimentale et raisonnée*, tom. ii. p. 393. Paris, 8°, 1773.



IX.

*Account of the new Electrical or Galvanic Apparatus of Sig. ALEX. VOLTA, and Experiments performed with the same.—W. N.*

FROM motives of delicacy to the inventor of the most curious and important combination hereafter to be described, I forbore giving an account of its construction and effects in the last number of this Journal, though it has now been a subject of great attention among philosophers for near two months. It appeared proper to avoid the publication of facts, originally flowing from the liberal communication of the worthy president of the Royal Society, until the paper of the inventor had been read to that learned body; and this could not be done till very lately, because the latter part of his memoir did not arrive till long after the first four pages.

The Right Honourable Sir J. Banks, Bart. P. R. S. having favored my friend Anthony Carlisle, Esq. with the perusal and consideration of these four pages at the latter end of last April, I had the pleasure to look them over with him, immediately after which he constructed an instrument according to Sig. Volta's directions. The experiments made with this will form part of the present communication; but in the first place, I shall endeavour to relate the leading particulars of the communication made to the Royal Society, which no doubt will hereafter appear at large in their Transactions.

The portion of letter which first arrived from Sig. Volta, is dated from Como in the Milanese, March 20, 1800. This, together with the subsequent parts, contains a detailed account of the instrument, of which the following is one of the most convenient forms.

Take any number of plates of copper, or which is better of silver, and an equal number of tin, or which is much better, zinc, and a like number of discs, or pieces of card or leather, or cloth \*, or any porous substance capable of retaining moisture. Let these last be soaked in pure water, or which is better, salt and water, or alkaline lees. The silver or copper may be pieces of money †. Build up a pile of these pieces; namely, a piece of silver, a piece of zinc, and a piece of wet card: then another piece of silver, a piece of zinc, and a piece of wet card: and so forth, in the same order (or any other order, provided the pieces succeed each other in their turn) till the whole number intended to be made use of is builded up. The instrument is then completed.

In this state it will afford a perpetual current of electricity, through any conductor communicating between its upper and lower plates; and if this conductor be an animal, it will receive an electrical shock as often as the touch is made, by which the circuit is com-

\* Woollen or linen cloth appear to be more durable, and more speedily soaked than card.

† Most of our philosophers have used half crowns for the silver plates. The zinc may be bought at 8d. per lb. at the White Lion in Foster Lane, and cast in moulds of stone or chalk. A pound makes twenty thick pieces of the diameter of half a crown, or 1.3 inches diameter.

pleted. Thus if one hand be applied to the lower plate, and the other to the upper, the operator will receive a shock, and that as often as he pleases to lift his finger and put it down again.

This shock resembles the weak charge of a battery of immense surface, and its intensity is so low, that it cannot make its way through the dry skin. It is, therefore, necessary that a large surface of each hand should be well wetted, and a piece of metal be grasped in each, in order to make the touch, or else that the two extremities of the pile should communicate with separate vessels of water, in which the hands may be plunged.

The commotion is stronger the more numerous the pieces. Twenty pieces will give a shock in the arms, if the above precautions be attended to. One hundred pieces may be felt to the shoulders. The current of electricity acts on the animal system while the circuit is complete, as well as during the instant of commotion, and the action is abominably painful at any place where the skin is broken.

That the energy of the apparatus is the effect of an electric stream or current, is proved by the condenser with which Sig. V. ascertained the kind of the electricity and obtained its spark. He finds the action strongest, or most pungent, on wounds on the minus side of the apparatus, or where the wounds give out electricity, a fact also observable in the common electric spark.

The theory of the learned inventor, if I rightly apprehend him, is, that it is a property of such bodies as differ in their power of conducting electricity, that when they are brought into contact they will occasion a stream of the electric matter. So that if zinc and silver be made to communicate immediately by contact, there will be a place of good conducting energy; and if they be made to communicate mediately by means of water, there will be a place of inferior conducting energy: and wherever this happens there will be a stream or current produced in the general stock of electricity. This is not deduced as the consequence of other more simple facts; but is laid down as a general or simple principle grounded on the phenomena.

As the current of electricity will be resisted by the different conductors, he remarks that the metals may touch in a single point, or be folded together; but that the humid surfaces must be more extended.

By many experiments, he finds that the consequences are the same whether the zinc and silver touch each other, or whether the communication be made by several different metals, provided the water be in contact with the zinc and the silver only.

Where zinc is used, salt water is preferable to alkaline lees, but the contrary when tin is made use of instead of the zinc.

The effect is much increased by elevation of temperature.

He was surprized to find that the galvanic flash of light was no greater with this apparatus than with a pair of plates; but it was produced when the conductor of the circuit was applied to any part of the face, or even to the breast. The strongest action was when the touching plate was held between the teeth, so as to lie upon the tongue. In this case



the lips and tongue were convulsed, the flash appeared before the eyes, and the taste was perceived in the mouth.

Two blunt probes were inserted in the ears, and the shock passed through the head, after which the communication was kept up. A peculiar sound, like crackling or boiling, was heard; but the author did not think it prudent to make this experiment repeatedly.

The sense of smell could not be excited, because, as Sig. V. remarks, this electricity cannot be made to diffuse itself in the air.

As the discs become dry, and lose their power, Sig. V. endeavoured to prevent this effect by inclosing the column in wax or pitch, and in this he has so far succeeded, that he has fitted up two columns of twenty pieces each, which have acted well for some weeks, and he hopes will for months.

The combination, which he thinks the most instructive, consists of a row of glasses or cups (not of metal) containing warm water or brine. Into each of these is plunged a plate of zinc and another of silver, not touching each other. From these plates respectively proceed tails or prolongations, which communicate with or touch the plates of the outer glasses in such a manner, that the zinc of the first cup communicates with the silver of the second; the zinc of the second with the silver of the third; the zinc of the third, &c. progressively and regularly through the whole row. The communication between the first and last glasses gives the shock, &c. The plates in the fluid are directed to be about an inch square; but the contacts above the water may be as small as the operator pleases.

Sig. Volta makes honourable mention of my conjectural theory of the torpedo\*. After remarking that my inductions were the most probable that the existing theory of electricity could at that time afford, he proceeds to make various objections needless to be here detailed, and then offers his own new and striking apparatus as more nearly resembling the torpedinal organ. I need not anticipate the reader in the happy points of resemblance between their structure and effects.

Thus far I have followed this able philosopher; who, to his former researches into the nature and laws of electricity, has now added a discovery which must for ever remove the doubt whether galvanism be an electrical phenomenon. But I cannot here look back without some surprize, and observe that the chemical phenomena of galvanism, which had been much so insisted on by Fabbroni†, more especially the rapid oxidation of the zinc, should constitute no part of his numerous observations.

On the 30th of April, Mr. Carlisle had provided a pile consisting of 17 half crowns, with a like number of pieces of zinc, and of pasteboard, soaked in salt water. These were arranged in the order of silver, zinc card, &c. which order I shall denote by saying, that the silver was undermost, that is to say, under the zinc; and I make this remark because some philosophers have used the expression that the silver was undermost when they used the order of silver, card zinc, &c. which, as the reader will easily perceive, is contrary to the order here spoken of. This is of no consequence to the effect, though it is material to a

\* Philosophical Journal, I. 358.

† Philosophical Journal, IV. 120.

clear understanding of the terms we use. This pile gave us the shock as before described, and a very acute sensation wherever the skin was broken. Our first research was directed to ascertain that the shock we felt was really an electrical phenomenon. For this purpose the pile was placed upon Bennett's gold leaf electrometer, and a wire was then made to communicate from the top of the pile to the metallic stand or foot of the instrument. So that the circuit of the shock would have been through the leaves, if they had diverged. But no signs of electricity appeared. Recourse was then had to the revolving doubler, described at page 95 of our present volume. The plate A was connected with the top of the electrometer and the silver end of the pile; and the plate B and ball were made to touch the top of the system by an uninsulated brass wire. The doubler had been previously cleared of electricity by twenty turns in connection with the earth. The negative divergence was produced in the electrometer. Repeated experiments of this kind shewed that the silver end was in the minus, and the zinc end in the plus state.

In all these experiments it was observed, that the action of the instrument was freely transmitted through the usual conductors of electricity, but stopped by glass and other non-conductors. Very early in this course, the contacts being made sure by placing a drop of water upon the upper plate, Mr. Carlisle observed a disengagement of gas round the touching wire. This gas, though very minute in quantity, evidently seemed to me to have the smell afforded by hydrogen when the wire of communication was steel. This, with some other facts, led me to propose to break the circuit by the substitution of a tube of water between two wires. On the 2d of May we, therefore, inserted a brass wire through each of two corks inserted in a glass tube of half an inch internal diameter. The tube was filled with New river water, and the distance between the points of the wires in the water was one inch and three quarters. This compound discharger was applied so that the external ends of its wire were in contact with the two extreme plates of a pile of thirty-six half crowns with the correspondent pieces of zinc and pasteboard. A fine stream of minute bubbles immediately began to flow from the point of the lower wire in the tube, which communicated with the silver, and the opposite point of the upper wire became tarnished, first deep orange, and then black. On reversing the tube, the gas came from the other point, which was now lowest, while the upper in its turn became tarnished and black. Reversing the tube again, the phenomena again changed their order. In this state the whole was left for two hours and a half. The upper wire gradually emitted whitish filmy clouds, which, towards the end of the process, became of a pea green colour, and hung in perpendicular threads from the extreme half inch of the wire, the water being rendered semi-opaque by what fell off, and in a great part lay, of a pale green, on the lower surface of the tube, which, in this disposition of the apparatus, was inclined about forty degrees to the horizon. The lower wire of three quarters of an inch long, constantly emitted gas, except when another circuit, or complete wire, was applied to the apparatus; during which time the emission of gas was suspended. When this last mentioned wire was removed, the gas re-appeared as before, not instantly, but after the lapse of four beats of a half second clock



clock standing in the room. The product of gas, during the whole two hours and a half, was two-thirtieths of a cubic inch. It was then mixed with an equal quantity of common air, and exploded by the application of a lighted waxed thread.

It might seem almost unnecessary to have reversed the order of the pile in building up, as reversing the tube must have answered exactly the same purpose. We chose, however, to do this, and found that when the zinc was at the bottom, its effects were reversed, that is to say, the gas still came from the wire communicating with the silver, &c.

We had been led by our reasoning on the first appearance of hydrogen to expect a decomposition of the water; but it was with no little surprize that we found the hydrogen extricated at the contact with one wire; while the oxygen fixed itself in combination with the other wire at the distance of almost two inches. This new fact still remains to be explained, and seems to point at some general law of the agency of electricity in chemical operations. As the distance between the wires formed a striking feature in this result, it became desirable to ascertain whether it would take place to greater distances. When a tube three quarters of an inch in diameter, and thirty-six inches long, was made use of, the effect failed, though the very same wires, inserted into a shorter tube, operated very briskly. The solicitation of other objects of enquiry prevented trial being made of all the various intermediate distances; but from the general tenor of experiments, it appears to be established, that this decomposition is more effectual the less the distance between the wires, but that it ceases altogether when the wires come into contact.

May 6.—Mr. Carlisle repeated the experiment with copper wires and tincture of litmus. The oxidating wire, namely, from the zinc side, was the lowest in the tube; it changed the tincture red in about ten minutes as high as the upper extremity of the wire. The other portion remained blue. Hence it seems either an acid was formed, or that a portion of the oxygen combined with the litmus, so as produce the effect of an acid.

It may be here offered as a general remark, that the electric pile with card, or with woollen cloth, continues in order for about two days, or scarcely three; that from a series of glasses set up by Mr. Carlisle, as well as from the pile itself, it appears that the same process of decomposition of water is carried on between each pair of plates, the zinc being oxidized on the wet face, and hydrogen given out; that the common salt is decomposed, and exhibits an efflorescence of soda round the edges of the pile, extruded, most probably, by the hydrogen: and that on account of the corrosion of the faces of the zinc, it is necessary to renew them previous to each construction of the pile. This may be done by scraping or grinding. I found it most convenient to lay the piece in a hole in a board, and give it a stroke with a float file, or file of which the teeth are not crossed. It might, perhaps, be less troublesome to clean them with diluted muriatic acid; but this I have not tried.

As the ample field of physiological research to which Mr. Carlisle's attention is directed, and the multiplicity of my own avocations, rendered it less convenient for us to pursue

our

our enquiries together, I constructed an apparatus for my own use. Zinc was laminated to the twenty-fourth part of an inch in thickness, and pure silver to the one-thousandth part of an inch, that is to say, as thin as our flattening mills can bring it.

Of these metals I made two sets, namely, sixteen pieces of silver of two inches in diameter, and sixteen pieces of 1.8 inch diameter, with their correspondent plates of zinc and wetted card. The small pile was first prepared, and whether it were that these thin pieces were more disposed to admit the water between the metallic faces of contact, or from whatever other cause it may have arisen, it did not appear by any experiment, that the whole set, though so greatly exceeding the pile of half-crowns in surface, was capable of doing more in the decomposition of water, or in communicating the shock. But this, with other facts, seems to shew, that the repetition of the series is of more consequence to this action, than the enlargement of surface; and also that the thickness of the plates, though it may be attended with convenience, most probably affords no addition to the force. I must also add, that I have no reason to recommend my pile, though at first sight it seemed to possess cheapness and convenience. The plates of zinc are too thin to bear frequent cleaning or renewing after corrosion of the surface, and the silver, though it is scarcely acted on in this situation, is too thin to be conveniently wiped or handled.

The spontaneous electricity of the doubler presented an objection to the strict fidelity of its results; whence I thought it desirable to give my pile a trial with the condenser. The foot or stand of my electrometer is a brass plate truly flat, and 3.8 inches in diameter. A piece of thin Persian silk was tied smoothly upon the face of this plate, and it was then placed upon another brass plate, upon which it was moved about horizontally, in order to accumulate electricity by friction; the electrometer itself being used as the handle by grasping the top. It was found that this treatment produced very weak signs of electricity when the electrometer was lifted up. The lower brass plate was then placed on the top of the small pile, and the condensing electrometer placed upon it. A communication was then made, by means of a wire from the lower or silver end of the pile to the upper plate of the condenser, or foot of the electrometer. In this situation it is evident, that the charge of the pile was employed in producing opposite states of electricity in the condenser, which would be shewn when the plates came to be separated. The wire of communication being taken away, the electrometer was lifted, and the leaves diverged and struck. It became necessary, therefore, to repeat the experiment, taking care to lift the electrometer more gradually. The divergence took place as before, and it was increased by presenting excited sealing wax towards the bottom of the electrometer. And as the top of the pile had by compensation diminished the same divergence, it is clear that the electricity of the top of the pile, *viz.* of the zinc, was contrary to that of sealing wax; that is to say, the zinc was in the plus state. After a number of repetitions of this experiment with the same invariable result, the pile was then carefully overfset, without disturbing the relative arrangement of its parts; so that the zinc was now at the bottom, and the silver at the top.



top. The electricity of the silver was then tried a number of times, by precisely the same process as before, and it exhibited an equal degree of intensity, but it was minus or negative. In one of these experiments, I certainly saw the spark at the time of completing the circuit, and afterwards with the same pile, when I was expressly looking for it. But it is less necessary to dwell on these facts, as the stronger combinations have exhibited this effect with much greater perspicuity.

The decomposition of water, and oxidation of metallic wire, gave birth to a variety of speculations and projects of experiments. Among others it became a question, what would be the habitude of metals of difficult oxidation. Two wires of platina, one of which was round, and one fortieth of an inch in diameter, and the other nearly of the same mass, but flattened to the breadth of one twenty-fifth of an inch, were inserted into a short tube of  $\frac{1}{4}$  of an inch inside diameter. When placed in the circuit, the silver side gave a plentiful stream of fine bubbles, and the zinc side also a stream less plentiful. No turbidness nor oxidation, nor tarnish appeared, during the course of four hours continuance of this operation. It was natural to conjecture, that the larger stream from the silver side was hydrogen, and the smaller oxygen. Thick gold leaf was tried with the same effects. A wire of brass was then substituted instead of one of the slips of gold. When the brass was on the minus, or silver side, the two gases were extricated for two hours, without oxidation as before; but when the brass was, by reversing the tube brought to the plus side, it became oxidized in the same manner as if both the wires had been brass. When the slips of gold were long subjected to this action, the extremity of the slip communicating with the zinc, acquired a coppery or purpleish tinge, which was deepest near the end. Whether this arose from oxidation of the gold, or of the copper, of which gold leaf contains about a seventieth part, cannot from this experiment be decided.

The simple decomposition of water by platina wires without oxidation, offered a means of obtaining the gases separate from each other. With this intention, Mr. Carlisle's pile of thirty-six was combined with my two sets of sixteen repetitions. His pile was built with the zinc uppermost, and mine in the reverse order; so that by connecting the upper plates the whole constituted one range, and the communications could be made from the bottom of the one to the bottom of the other. The two platina wires were made to protrude out of two separate tubes, each containing a little water, and through the opposite corks of each were passed copper wires of communication. These tubes were slightly greased on the outside to prevent their becoming damp; and in this state the extremities, armed with the platina, were plunged in a shallow glass vessel of water, in which two small inverted vessels, quite full of water, were so disposed, that the platina of one tube was beneath one vessel, and the platina of the other tube was beneath the other, the distance between their extremities being about two inches. The copper wires of these tubes respectively were made to communicate with the extremities of the intire pile of sixty-eight sets. A cloud of gas arose from each wire, but most from the silver, or minus side. Bubbles

bles were extricated from all parts of the water, and adhered to the whole internal surface of the vessels. The process was continued for thirteen hours, after which the wires were disengaged, and the gases decanted into separate bottles. On measuring the quantities, which was done by weighing the bottles, it was found, that the quantities of water displaced by the gases, were respectively, 72 grains by the gas from the zinc side, and 142 grains by the gas from the silver side; so that the whole volume of gas was 1.17 cubic inches, or near an inch and a quarter. These are nearly the proportions in bulk, of what are stated to be the component parts of water. The gas from the zinc side, being tried with one measure of nitrous gas, contracted to 1.25, and did not contract more by the addition of another measure; the gas from the silver side by the same treatment contracted to 1.6. The air of the room, on trial, contracted to 1.28. From the smallness of the quantity no attempt was made to detonate the air from the zinc side, but a portion of that from the silver side, being mixed with one third of atmospheric air, gave a loud detonation.

Upon the above it may be remarked, that it does not seem probable that oxygen was afforded by both wires, but that they were mixed by the circumstances of the experiment. For the gases being extricated in extremely minute bubbles beneath the inverted vessels, caused a slow ascending current consisting of water mixed with those bubbles, many of which were undoubtedly too small to be discerned. This ascending current gave out as much of its gas at the top of the vessel, as had time to conglomerate; but the extremely minute bubbles would return in the descending current, and be repeatedly carried up before this effect could take place. Such a continual circulation, or stream, the lower part of which passed down into the faucer, must at length have occasioned the whole mass of fluid to become replete with these minute bubbles, which would break at the open surface, and be lost, or attach themselves to the sides of the vessel, as was seen to be the case. What proportion may have thus disappeared is uncertain; but it is highly probable, that one consequence of the imperfections of our apparatus, was to occasion both the inverted vessels to become receptacles for the gases from both wires indiscriminately; though most plentifully in each, from the wire immediately beneath its mouth. If this reasoning may be admitted for the present, till the experiment is repeated in closed vessels, it will be fair to reckon the whole diminution on both the quantities. The whole diminution was 1.15, whence it would follow, that the purity of the oxygen estimated in Priestley's manner, would be expressed by the number 0.85.

On account of the length of this communication, I shall at present forbear to enter into any considerations of theory, but shall conclude with a concise mention of the effects of a pile of one hundred half crowns, and a chemical incident, which appears to be the most remarkable of those which I have yet observed.

The pile was set up with pieces of green woollen cloth soaked in salt water. It gave severe shocks, which were felt as high as the shoulders. The transition was much less forcible through a number of persons, but it was very perceptible through nine. The spark



spark was frequently visible when the discharge was made in the dark, and a gleam of light was also, in some instances, seen about the middle of the column at the instant of the explosion. The assistants were of opinion that they heard the spark.

The extrication of the gases was rapid and plentiful by means of this apparatus. When copper wires were used for the broken circuit, with muriatic acid diluted with 100 parts of water in the tube, no gas, nor the least circulation of the fluid was perceived, when the distance of the wires was two inches. A short tube, with two copper wires very near each other in common water, was made part of the circuit, and shewed by the usual phenomena, that the stream of electricity was rapidly passing. The wires in the muriatic acid were then slid within a third of an inch of each other. For the sake of brevity, I avoid enumerating the effects which took place during several hours, and simply state; that the minus wire gave out some hydrogen during an hour, while the plus wire was corroded, and exhibited no oxide; but a deposition of copper was formed round the minus, or lower wire, which began at its lower end: that no gas whatever appeared in this tube during two hours, though the deposition was going on, and the small tube shewed the continuance of the electric stream; and that the deposition at the end of four hours formed a ramified metallic vegetation, nine or ten times the bulk of the wire it surrounded.

In this experiment it appeared, that the influence of electricity increasing the oxidability of the upper wire, and affording nascent hydrogen from the lower, caused the latter to act as the precipitant of a solution of one and the same metal.

We are in want of a measure of the intensity of the action of these machines. Will this be derived from the quantities of water decomposed, or of gas extricated under like circumstances in given times? Or from any change of temperature? Or what other commensurate incident?—Mr. Carlisle has not found that the water in the tube, while under this agency, did produce the slightest effect on a very small and delicate thermometer.

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## X.

*Some Experiments and Observations on Galvanic Electricity. By Mr. W. CRUICKSHANK, Woolwich. Communicated by the Author.*

IN subjecting a number of fluids to the action of galvanism, several facts have been discovered, which to me, at least, are perfectly new, and which appear to throw some light on the nature and powers of this new influence.

I shall, therefore, without any further apology, give a brief detail of some of the most important, hoping that they may prove acceptable to those who are employed in the same pursuits.

I shall not give any particular account of the apparatus employed, being a pile, and not differing materially from that in use. I shall only just observe, that it consisted of plates of zinc and silver, of about 1.6 inches square, and that the number of each employed in the following experiments varied from 40 to 100, according to the power required.

I found that a solution of the muriate of ammonia answered better for moistening the interposed papers than common water.

When the machine was in full action, sparks, which were perfectly visible in the day time, could be taken at pleasure, by making a communication in the usual way between the extremities of the pile, and a small report or snap could be heard; the shock given at that time was very strong, and a gold leaf electrometer, placed in the circle of communication, was very sensibly affected: these circumstances, some of which I believe have been already ascertained by Messrs. Nicholson and Carlisle, shew the strong resemblance of this influence to electricity. These gentlemen have likewise discovered that galvanism decomposes water with much greater facility than electricity, but with phenomena somewhat different.

*Experiment 1.*—A quantity of common water was introduced into a glass tube, being confined at each end by corks, but perfectly at one by a cement of rosin and bees'-wax: pieces of silver wire were passed through the corks, and brought within an inch of each other in the fluid, their other extremities being at the same time connected with these of the machine or pile, one with the lower zinc plate, and the other with the upper silver plate. In future, to avoid circumlocution, I shall call the wire attached to the silver plate, the silver wire, and the other the zinc wire. The tube was then placed upright in a cup containing water, with the uncemented end downwards. As soon as the communication was made between the extremities of the pile by the wires, a quantity of small air bubbles began to ascend from the end of the wire connected with the silver, as observed by Messrs. Nicholson and Carlisle; but a white cloud at the same time made its appearance at the one proceeding from the zinc, or the zinc wire. This cloud gradually increased, and assumed a darker colour, and at last it became purple, or even black. A very few air bubbles were likewise collected upon and ascended from this wire, but when the machine was in full force, a considerable stream could be observed.

The gas was collected, and found to be a mixture of hydrogen and oxygen, in the proportion of three parts of the former to one of the latter. No great dependence, however, was placed upon this in point of accuracy. The zinc wire was found to be much corroded, and looked as if a considerable portion of it had been dissolved. As the cloud which was formed around this wire became purple on exposure to the light, I suspected it might be luna cornea, or muriate of silver proceeding from the silver, which had been somehow dissolved, and afterwards precipitated in this state, by the muriatic salts in the common water. This led to the following experiments:

*Experiment 2.* The glass tube was now filled with distilled water, to which a little tincture of litmus was added, when the communication was made by the wires as in the for-

mer.



mer experiment, a quantity of gas arose from both wires, but in the greatest quantity from that connected with the silver. In a few minutes a fine red line, extending some way upwards, was perceived at the extremity of the zinc wire; this increased, and in a short time the whole fluid below the point of this wire became red; the fluid, however, above the silver wire, looked of a deeper blue than before, the slight tinge of purple being destroyed.

*Experiment 3.* I next filled the tube with distilled water, tinged with the tincture of Brazil wood; it was no sooner placed in the circle of communication, than the fluid surrounding the silver wire, particularly towards its extremity, became purple, and this tinge increased so fast, that the whole fluid surrounding this wire, and occupying the upper part of the tube, soon assumed as deep a colour, as could be produced by ammonia.

The portion of the fluid in contact with the zinc wire became very pale, and almost colourless, nor could the purple tinge extend below its upper extremity. From these experiments it would appear, that an acid, probably the nitrous, is produced at the wire proceeding from the zinc, and an alkali, probably ammonia, at that in contact with the silver. These facts sufficiently explain the action upon the silver wire, and the nature of the whitish cloud proceeding from it, and afterwards becoming purple. When lime-water was employed instead of common, or distilled water, the wire was likewise acted upon, but in a less degree, and the cloud had at first an olive colour, exactly resembling the precipitate of silver by lime-water.

The quantity of silver dissolved or corroded, if I may use the expression, in these experiments, was very considerable, and where common, or distilled water had been employed, a small portion of it remained in solution, which was discovered by the addition of the muriatic acid. Indeed a much larger quantity would probably have been suspended, had it not been for the alkali generated at the same time, and which manifestly produced a precipitate at, or near, the upper extremity of the zinc wire, where after a certain time, a dark zone or stratum was always formed.

*Experiment 4.* It is a well known fact, that hydrogen gas when heated, or in its nascent state, reduces the calces of the metals; I expected, therefore, that by filling the glass tube with a metallic solution, I might be enabled to separate the hydrogen from the oxygen gas, and thus procure the latter in its simple or pure state. With this view the tube was filled with a solution of the acetite of lead, to which an excess of acid was added, to counteract the effects of the alkali. When the communication was made in the usual way, no gas could be perceived, but after a minute or two, some fine metallic needles were perceived at the extremity of the wire connected with the silver. These soon increased, and assumed the form of a feather, or rather, that of the crystals of the muriate of ammonia. The lead thus precipitated was perfectly in its metallic state, and very brilliant; a little gas escaped from the wire connected with the zinc, and it was considerably corroded as usual.

A solution of the sulphate of copper was next employed, and with the same result, the

copper being precipitated in its metallic form by the wire connected with the silver. In this instance the metal did not crystallize, but formed a kind of button at the end of the wire, which adhered so completely to the silver, that it was found impossible to separate it.

The most beautiful precipitate, however, was that of silver from its solution in the nitrous acid. In this case, the metal shot into fine needle-like crystals articulated, or joined to each other, as in the *Arbor Dianæ*.

What became of the oxygen gas usually produced in these experiments?

*Experiment 5.* A quantity of pure water mixed with distilled vinegar was introduced into the tube, and placed in the circle of communication; some gas was disengaged from the silver wire, but no cloud appeared at the extremity of the zinc. After some time, however, a quantity of metallic silver was precipitated by the silver wire, and this precipitate at last became very copious; a perfectly similar effect was produced, when the tube was filled with very dilute sulphuric acid; in these cases the precipitated silver had the appearance of shining scales, like that thrown down by copper in the usual way. It may be proper to observe, that in all these precipitations and reductions, nothing but wires of pure silver were employed. The results in this last experiment were exactly what was expected; the vinegar prevented the alkali from precipitating the silver, dissolved by the generated acid; in consequence of which, when a sufficient quantity of the metal was taken up, it was again thrown down by the silver wire in its metallic form.

*Experiment 6.* A solution of the muriate of ammonia being introduced into the tube, and exposed to this influence, a little gas was disengaged from the silver wire, while the zinc one was incrustated with a substance which soon became black, and was found to be *luna cornea*. The liquor which remained in the tube after the operation had been finished, was highly alkaline, and smelled strongly of ammonia; common salt was decomposed in a similar manner. This experiment accounts for the decomposition of the muriate of soda and ammonia, which always takes place when the papers in the pile are moistened with a solution of these salts.

A solution of the nitrate of magnesia appeared to be likewise decomposed by this process, for after some time, a white powder resembling magnesia, was precipitated on the surface of the silver wire; very little gas was disengaged.

*Experiment 7.* In order to ascertain how far this influence might be carried, provided the circle of communication was complete, two tubes were employed, and connected by a silver wire passing through corks as in the figure, where A, B, represent the two tubes, and C the connection wire; the tubes were filled with water and secured by corks; two other wires D and E, being then passed through these corks, the arc D was connected with the silver, and the other with the zinc, at the extremity of the pile. A quantity of gas as usual was disengaged at the extremity of the silver wire D in A, and the portion of the connecting wire C in the same tube was partly dissolved, and as mentioned in experiment 1; but the other portion of the same wire in the tube B gave out gas, while the communicating

zinc



zinc wire E was corroded. And I make no doubt that a similar effect would be produced, if any number of tubes were connected in a similar manner, by which means a large quantity of gas might be procured in a short time.

Besides silver wires, I likewise employed those of copper or iron, and it did not appear that these were more corroded or acted upon than the silver; indeed, in some of the above experiments, not less than half, or three quarters of an inch of the wire was intirely consumed. The copper wire connected with the zinc gives out a greenish blue substance resembling the nitrate of copper with excess of the metal, or when part of the acid has been expelled by heat, &c. In examining the gas which was procured at different times, I always found it mixed with a little oxygen gas, but sometimes this did not exceed  $\frac{1}{2}$  of the whole in bulk; however, I paid but little attention to this part of the process, for as my wires were always corroded, no conclusion with regard to the composition of water could be drawn from it.

At present I shall only further observe, that by making the galvanic influence pass through a quantity of distilled water confined in a tube over mercury, for about 48 hours, a manifest diminution of the fluid was perceived.

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## SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

### *Magnetic Dip at Prince of Wales's Island.*

MR. George Thomas Staunton, in a letter to his father, Sir George Staunton, Bart. dated Prince of Wales's Island, Nov. 4, 1799, states, that "our landing at this place has enabled me to make use of the dipping needle, which I brought on shore, and found that by the means of several observations, the south pole of the needle dipped five degrees ten minutes."

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### *Infusibility of Tungsten.*

The specific gravity of Tungsten has not been ascertained with precision, on account of the extreme difficulty of fusing this metallic substance. Guyton, in a fire urged by the blast of three pipes to 185 degrees of the pyrometer, obtained a well rounded piece of 35 grammes. But it broke in the vice, and exhibited a central portion, which was only agglutinated, and soon acquired a purple colour by exposure to the air. He found that the well!

well fused portion had no higher specific gravity than 8.3406, and he concludes from the infusibility and brittleness of this metal, that it affords little promise of utility in the arts, except in metallic alloys, or by virtue of the property which its oxide possesses, of affording fixed colours, or giving fixity to the colours of vegetables.

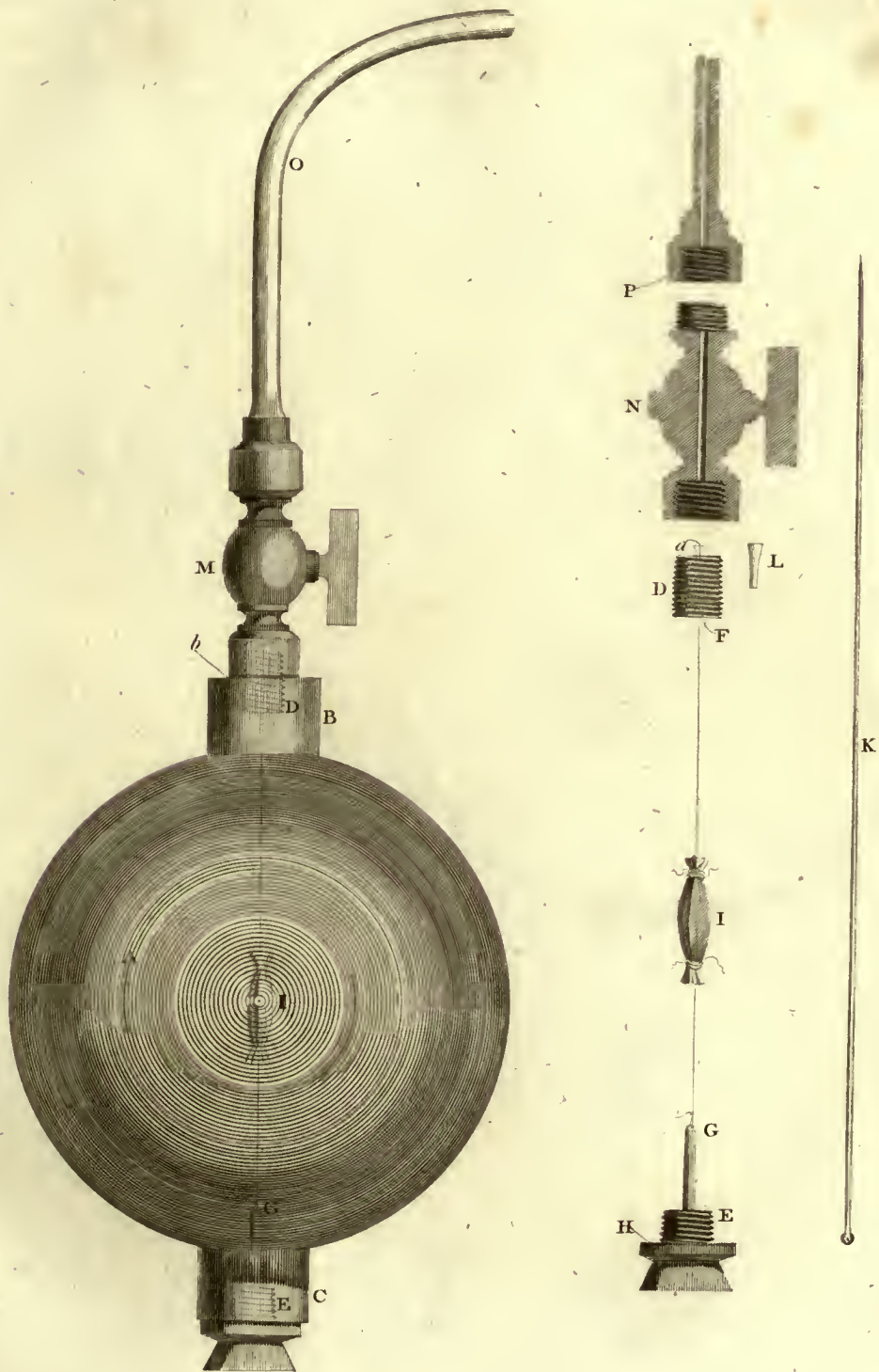
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The *Geometrie descriptive* of Gaspard Monge is published at Paris in quarto, with 25 plates, by Bernard, and likewise a new edition of his *Statique*.

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The reference to a drawing in Mr. Cruickshank's paper, in our present Number, relates to a sketch which, on account of the late reception of the copy, could not be inserted in the plate. It consists simply of two tubes, forming what I have called the broken circuit in the manner mentioned in my experiment, where a tube with water was connected with another containing marine acid, and both set to work at the same time. It seems very probable, that a large quantity of gas might be thus extricated; but from the circumstances of the last mentioned experiment, it seems apparent that the power of the apparatus in each individual tube will be less the greater the number.

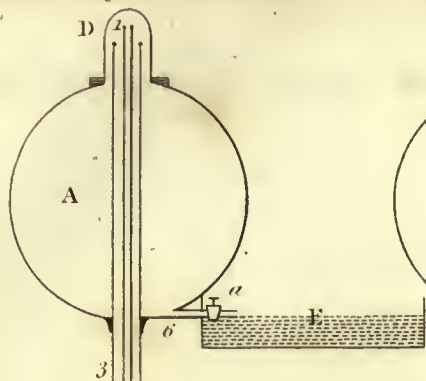




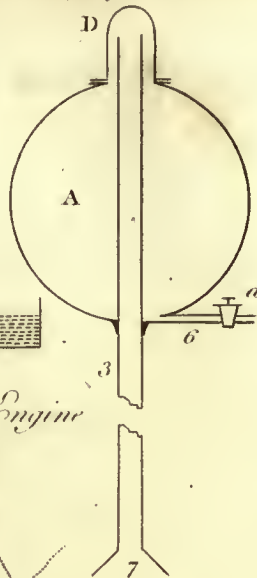




*Fig. 2.*



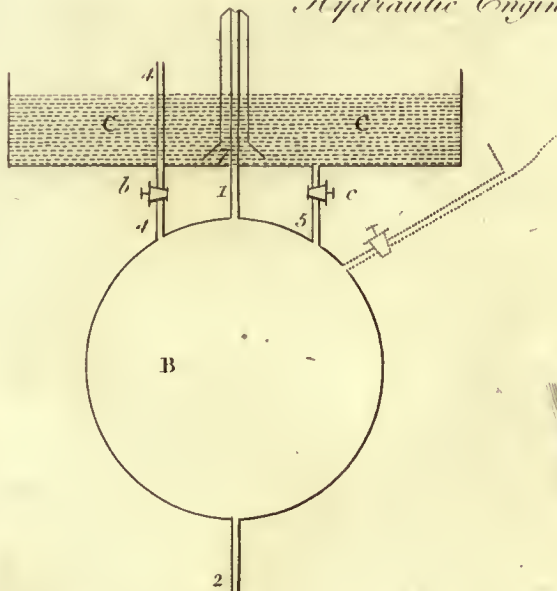
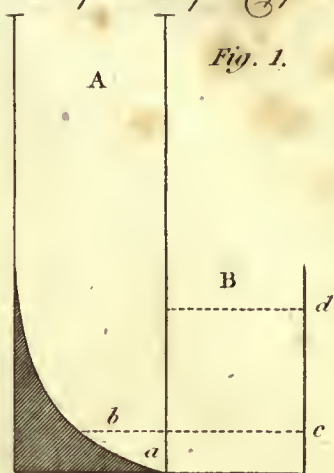
*Fig. 3.*



*Hydraulic Engine*

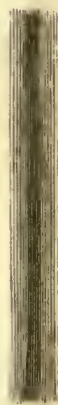
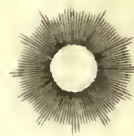
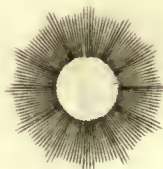
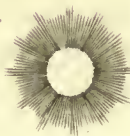
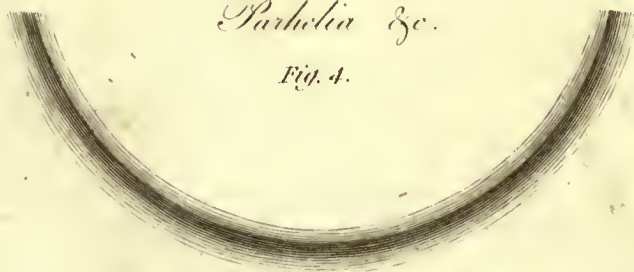
*New furnace for Copper*

*Fig. 1.*



*Parhelia &c.*

*Fig. 4.*







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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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AUGUST 1800.

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ARTICLE I.

*Experiments on the Production of Air by the Freezing of Water. By JOSEPH PRIESTLEY,  
LL.D. F.R.S. &c. &c. Communicated by the Author.*

IN 1793, when I was in England, I published an account of a course of experiments on the generation of air from water, and, after my arrival in this country, I published a sequel to them. The result of the whole was that, after all air had, in every known method, been extracted from any quantity of water, whenever any portion of it was converted into vapour, a bubble of permanent air was formed, and this was always phlogificated. The process with the torricellian vacuum I continued some years, and found the production of air equable to the last.

The necessary inference from these experiments is either that water is convertible into phlogificated air, or that it contains more of this air intimately combined with it than can, by this process, be extracted in any reasonable time.

Finding that no air is contained in ice that is free from visible bubbles, I thought to ascertain the truth of one of those hypotheses by exposing to cold a quantity of water from which I had, by repeated processes with the torricellian vacuum, expelled all the air that I possibly could; thinking that if it really contained no air, it would appear by the

ice being perfectly solid, so that when it was melted, no air could be got from it. This experiment I repeated several times, but always found that, though the outside of this ice was perfectly transparent, and free from air bubbles, the central parts were opaque; and though there were no distinct air bubbles there, yet when it was melted, a great number issued from it. The whole quantity of air, however, was not more than might have been produced from the same water in the other processes in a reasonable time; and in them the production of air had no limit.

Disappointed in my expectation of getting by this means ice perfectly free from air (which, when a large quantity of water freezes very slowly it is easy to do, the air contained in it retiring from that which is frozen to that which remains fluid) I dissolved ice that was perfectly transparent, and therefore free from air, in vessels containing mercury, and exposed it to frost a second time. But I always found that when the whole of it was frozen, the extreme parts were transparent, the central parts were opaque, and when dissolved, yielded air. Though I repeated this process ten or a dozen times with the same water, always letting out the air that was procured by freezing, presently after it was extricated, and before it could have been re-absorbed; yet on exposing it to another freezing, I never failed to get more air, and the harder the frost was, the more air I procured.

As there is an evaporation from ice no less than from water, the interstices made by the crystallization of the water, when it is converted into ice, will soon be filled with vapour; and this vapour, like that which is formed by heat, becomes the basis, I suppose, of a quantity of air. Since, however, the most transparent ice swims in water, this also must have interstices, but they contain no air; being such as exist in the most solid bodies in which (gold itself not excepted) the component particles are not in perfect contact, since they are reduced into less dimensions by cold.

As the vessels I made use of in these experiments were either cylindrical jars, or conical wine glasses, and, consequently, the bubbles of air procured by freezing were exposed to a considerable surface of water, and would in time (though not, I found, in the course of a day) have been absorbed by the water more free from air, I procured glass vessels of a conical form, terminating in narrow tubes, into which the air dislodged from the ice might ascend, and not be subject to be absorbed. I was so fortunate as to have several vessels of this form, and they completely answered my purpose for five or six processes.

These vessels were first filled with mercury, and then I introduced into them a quantity of water freed from air by previous freezing; and when, after exposure to frost the ice was melted, the air dislodged from the ice ascended into the narrow tubes, and remained without any sensible diminution of bulk several days; and every time that the water was exposed to the frost, an addition was made to it. At length, however, though the vessels were very strong, and contained much mercury, which, by its tendency to descend, would give the water room to expand with the less danger of breaking the vessel, none of them served for more than the number of processes above-mentioned.

After



After the breaking of my glass vessels, I got other cylindrical ones made of *iron*, seven or eight inches in height, and near three inches wide at the bottom, the upper orifice being closed with a cork and cement, in the centre of which was a glass tube, the diameter of which was about the fifth of an inch. And as this glass tube was in the greatest danger of breaking by the freezing of the water, and this had happened several times in the former experiments, notwithstanding all my care to guard them from the frost, I now made use of snow and salt to freeze the water in the iron vessel only, placed in a vessel of mercury, having been previously filled as the glass vessels had been.

The water on which I now operated was about three ounces, and it had been made as free as possible from air by previous freezing. With this apparatus I repeated the process of freezing nine times, without changing the water; and the last portion of air that I procured in this manner was as great as any of the preceding; so that there remained no reasonable doubt, but that air might be produced from the same water, in this manner, *ad libitum*; and having got near two inches of air in the glass tube, I put an end to the experiment; and examining the air, I found it to be wholly *phlogificated*, not being affected by nitrous air, and having nothing inflammable in it.

During the process of freezing, the air in the tube was generally compressed into one-fifth of its usual bulk; but when I began to thaw the ice, which I did by means of hot water in the place of the freezing mixture, it soon expanded to its former dimensions, and no sensible portion of it was absorbed during the whole process, which was about a week. Sometimes the violence of the pressure, occasioned by the expansion of the water in freezing, would force a little water out of the vessel between the cork and the glass tube, or the iron vessel, which presently became ice. This I always carefully removed, and applied fresh cement to the place, to prevent the introduction of any air from without, before I began to melt the ice. And that no external air had entered, was evident both from the manner in which the air was produced after the water recovered its fluidity, and from the quality of it when examined after the process.

In the course of the experiments with the glass vessels, a phenomenon occurred which was wholly unexpected by me, and which was very amusing. Having left the vessels filled in part with water, and in part with mercury, in the evening, I generally found them in the morning seemingly quite full of mercury, every part of the ice within the vessel being covered with it. This must have been occasioned by a vacuum having been formed between the glass and the ice, and into this space mercury had been drawn up on the principle of the capillary tube. When this was not the case, the interstices of the ice towards the centre were filled with thin laminæ of mercury, which also exhibited a curious appearance.

Sometimes, when there was no mercury between the glass and the ice, an interstice was made between them when they were placed within the influence of the fire. In these circumstances I have seen the mercury drawn up to the height of several inches. As this space became enlarged by the increase of the heat, the laminæ of mercury were con-

tracted, till coming into the form of balls, too heavy to be supported, they fell down to the mass of mercury in the basin.

The most obvious inference from these experiments is, that water, when reduced by any means to the state of vapour, is in part converted into phlogisticated air, and that this is one of the means provided by nature to keep up the equilibrium of this constituent part of the atmosphere; as the influence of light on growing vegetables is the means of recruiting the other parts of it, viz. dephlogisticated air; and both of them are subject to absorption and diminution in several natural processes. Inflammable air, I have also shewn to be convertible into phlogisticated air; so that this is another method of supplying the atmosphere with this ingredient in its composition.

That water contains phlogiston, I have shewn to be probable from several considerations, especially that of its resembling metals in their property of being conductors of electricity. For these substances certainly contain phlogiston, if there be any such thing. Mercury also becomes super-phlogisticated by agitation in water, and this without limit, and without changing the water or the mercury; and the remaining water contains no more oxygen than before. For the air expelled from it is not more pure, but considerably less so, and it is perfectly free from acidity.

I would further observe, that these experiments, which prove the conversion of water into phlogisticated air, are inconsistent with the antiphlogistic theory, which makes water resolvable into dephlogisticated and inflammable air; but that they are highly favourable to the hypothesis of water being the basis of every kind of air, the difference between them depending upon the addition of some principles which we are not able to ascertain by weight. Also, if any species of air be entitled to the appellation of *hydrogen*, it is not inflammable, but phlogisticated air.

## II.

*On the Absorbent Powers of different Earths. By Mr. JOHN LESLIE. Communicated by the Author.*

**I**N completing my researches in hygrometry, I have been led to examine the habitudes of the earthy bodies with respect to moisture. I had already discovered, that animal and vegetable matters, like the saline and deliquescent substances, attract humidity by a force altogether distinct from that of capillary absorption, and attended with diminution of volume, evolution of heat, and the other concomitants which mark a real change of constitution. And while the empire of chemical agency seemed extending on every side, it was reasonable to doubt if the earths themselves continued entirely inert and passive under the atmospherical influences. My suspicions were fully confirmed by experiment. *All the earths and stony bodies eminently attract moisture from the air, and this with different degrees*



*degrees of force, modified also by temperature, which affects the measure of all combinations.* The facts thus obtained, though not of a brilliant nature, appear highly instructive, and if prosecuted with attention, might throw some light on the obscure theory of vegetation, and suggest useful improvements in agriculture and rural æconomy. I give this early notice of the little which I have done, as it seems to diverge from that line of inquiry which I had prescribed to myself, and as I shall not at least for the present, pursue it farther. But I earnestly invite chemists and naturalists to resume a subject which promises such an easy and abundant harvest.

My procedure was this:—The earths, or the stony matters grossly pounded, were dried thoroughly before a good fire, and immediately introduced into phials, which were stopped up and set aside to cool. The first object was to ascertain, whether the degree of heat to which they were subjected in drying, would affect their absorbent quality. I soon perceived, however, that the results were quite regular and uniform. In fact, the powder is dried in this case not by the action of the hot air on its surface, but by the heat penetrating the mass, and communicating to the aqueous particles the disposition to assume the vaporous state, with the corresponding elasticity of steam, which beyond the boiling point increases with most rapid progression, and which enormous distensive power soon overcomes the obstinate adhesion to the earthy basis. Any heat almost between the 100<sup>th</sup> and 300<sup>th</sup> degree of the centigrade scale, will bring the substances into the proper condition. This desiccated powder is strewed on the bottom of a saucer holding an hygrometer, over which a small receiver is inverted. In a few minutes the instrument marks the highest degree of dryness produced. Here are a few of the principal results indicated by this mode, at the temperature of 16° centigrade.

Free stone . . . . .	37°
Fine sea sand . . . . .	40
Marble . . . . .	47
Common clay . . . . .	68
Sea sand, lately cultivated . . . . .	72
Sandy schist . . . . .	75
Bog earth . . . . .	77
Rotten whinstone . . . . .	78
Garden mould . . . . .	80

It is remarkable that marble and quicklime produce exactly the same effect, and that in general no sensible difference is perceived between the pure earths and their carbonates. The great absorbent power of the argillaceous, compared with the silicious bodies, also deserves particular notice. But the cultivated soils possess that power in the most conspicuous degree. Garden mould stands at the top of the scale, and even sea sand, which causes a dryness of 40° only is, by a few years' tillage, rendered capable of producing that of 72°. Nor is it the operation of manure, for the single effect of this material is inferior to that of most of the earths. It seems highly probable, that the fertility of soils depends chiefly on their

their disposition to imbibe moisture. Manures perhaps act as stimulants merely, the carbonaceous matter of plants is derived from the atmosphere, and the earth affords expansion for the fibrils of the roots, and supplies them with the aqueous element. Rotten whinstone constitutes the body of the soil in this fertile county, and it manifests among the stony compounds a superior degree of the power of absorption.

Torrefaction seems remarkably to diminish the faculty of the earthy substances to attract moisture. Clay, roasted in a strong fire, from 68 gives only 35 degrees by the hygrometer, and after being urged in a blacksmith's forge, affords no more than 8; whinstone, which in ordinary cases has a power equal to 78°, shews only 23°, after exposure to the fierce heat of a forge. Nor is the effect occasioned by any partial or incipient vitrification, for sandstone, which has undergone a violent ignition, shews likewise a similar change of property. If bodies therefore suffered no alteration of contexture from the impressions of the atmosphere, geologists would be furnished with one certain criterion, to decide whether a fossil owed its formation to the agency of fire or water. It is almost superfluous to observe, how vague and inconclusive are the arguments usually employed by the contending sects of Neptunists and Plutonists.

But although the mode of experimenting above described sufficiently marks their discriminating qualities, it does not exhibit the full effect of the desiccated powders; since, the hygrometer and the earth being included together, while the latter abstracts moisture from the air within the receiver, the former by its wetted ball continually imparts humidity, which notwithstanding the extensive surface of absorption, must always enfeeble the performance in proportion to the time of action. The most accurate way of experimenting, is to throw the powders, either from the phials, or directly on its being withdrawn from the fire, into a large flattened glass vessel with a narrow neck, which is corked up for several hours, and the condition of the included air may then be examined by letting down a small hygrometer suspended by a thread from the stopper. For want of more suitable apparatus, I used wine decanters, and having provided myself with specimens of the primitive earths, I began with determining their relative absorbent powers. The results at the same temperature, of 16° centigrade, were as follow:

Carbonate of strontian	23°
Carbonate of barytes	32
Quartz	40
Marble	70
Carbonate of magnesia	75
Alumine	84

This furnishes another proof that strontianite is an independant earth, since it stands apart from the rest. Quartz in small rounded fragments, like pigeon's eggs, picked up on the beach, gave almost the same result as when reduced to a powder. In general, the only use of pounding the stones is to accelerate their performance. It might be presumed, that the mixtures of those earths would produce intermediate effects; yet equal parts of flint and



and alumine gave as much as the latter singly. But the *quantity* of absorption must be distinguished from its *intensity*. Thus strontianite, barytes, and filex, are quickly saturated with moisture, while magnesia and alumine continue to imbibe it for a considerable time. The effects in mixture depend on those qualities combined: after the filex has ceased to act, the alumine perseveres, and through the intermediate agency of the air, divests the former of the minute portion of humidity which it had attracted. But the compounds of the simple earths are still more remarkable in their affection to moisture, since it surpasses that of their ingredients. Thus, sea sand gives  $70^{\circ}$ ; whinstone,  $80^{\circ}$ ; though it consists one half of filex, and the other half of alumine, and the oxide of iron\* in nearly equal parts; and pipe clay, which contains a large proportion of filex,  $85^{\circ}$ . It is evident, that the absorbent power of earths depends as much on their mechanical condition, as on the species of matter of which they are composed. Whatever tends to harden them diminishes their effect, and the contrary. Hence the reason why the action of fire impairs their desiccating quality. Quartz, urged in a blacksmith's forge, gives only  $19^{\circ}$ ; but the same powder, after being soaked a week in water, gave  $35^{\circ}$ ; and most probably it would have in time recovered its whole original power. The process by which nature gradually divides, softens, and disposes stony bodies to absorb moisture, is beautifully illustrated in the instance of whinstone or basalt. A piece of solid whinstone gave 80 degrees by the hygrometer, another piece rotten and crumbling, gave  $86^{\circ}$ ; but another portion of the same rock already reduced to mould, afforded  $92^{\circ}$  †. The ameliorating effects of culture is exemplified in sea sand: fine sand caused a dryness of  $70^{\circ}$ ; sand collected from the paths of a sheep walk near the beach  $78^{\circ}$ ; the same sand, lately brought into cultivation  $85^{\circ}$ . Still these effects are inferior to that of garden mould, which amounts to  $95^{\circ}$ , and to which decomposed whinstone approaches the nearest. Comparing those facts with the property lately discovered by Humboldt, that the earths attract the oxygenous portion of the air, we farther perceive the intimate concatenation of changes which maintain this mundane system. The lap of nature is prepared for the reception and developement of the vegetable germs, exhibiting in a reciprocating series of growth and decay, the ever renovating forms of organic bodies through endless successions of cycles.

Sulphuric acid nearly doubles its desiccating power at every 15 degrees of elevation of temperature, that at zero being  $60^{\circ}$ . The several earths appear to follow progressions not much different.

The relative effects of different soils may be ascertained more easily, by altering the construction of the hygrometer. Connect the two balls by a long bent tube passed through two corks, and cover and wet them both. In this state they are introduced into the wine

\* The oxide of iron and zinc which I have tried, shewed less absorbent powers than the earthy bodies.

† Those specimens were taken from a remarkable spot on the summit of Largo Lau, an elegant conical mountain, which rises behind this place to the height of about 800 feet above the level of the sea. The whinstone parts into small hexagonal columns, of which one of the sides is sometimes almost effaced.

decanters containing the two earths to be examined; the rise or depression of the liquor will indicate which has the greatest absorbent power, and the measure of its excess. I constructed such an instrument, but have not yet pushed the inquiry to any extent.

Before I conclude, I must notify that the desiccating quality of flannel may be employed for the graduation of the hygrometer, instead of that of the sulphuric acid. A large piece of flannel dried well at a fire, laid between the leaves of a book to cool, and then placed in folds within a large receiver, will produce a dryness of  $80^{\circ}$  at the temperature of  $16^{\circ}$ . Those, however, who wish to purchase the hygrometer or photometer, may apply to Mr. Cary, optician, in the Strand.

JOHN LESLIE.

*Largo, Fifeshire, June 9, 1800.*

### III.

*On a New Fulminating Mercury. By EDWARD HOWARD, Esq. F. R. S.*

*(Continued from page 178.)*

**H**AD the alchemist proposed to fix water by the same apparatus, the nest of boxes must, I suppose, have likewise been ruptured; yet it does not follow that the explosion would have been so tremendous: indeed it is probable that it would not, for if (as Mr. Kirwan remarked to me) substances which have the greatest specific gravity, have likewise the greatest attraction of cohesion, the supposition that the vapour of mercury exceeds in expansive force the vapour of water, would agree with a position of Sir Isaac Newton, *that those particles recede from one another with the greatest force, and are most difficultly brought together, which upon contact cohere most strongly* \*.

### SECTION IX.

Before I attempt to investigate the constituent principles of this powder, it will be proper to describe the process and manipulations which, from frequent trials, seem to me best calculated to produce it.

100 grains, or a greater proportional quantity, of quicksilver (not exceeding 500 grains †) are to be dissolved, with heat, in a measured ounce and a half of nitric acid ‡. This solution being poured cold upon two measured ounces of alcohol §, *previously* introduced into

\* Newton's Optics, p. 372, 4th Ed. Lond. 1730.

† The reason of this limitation is not on account of any danger attending the process; but because the quantities of nitric acid and alcohol required for more than 500 grains, would excite a degree of heat detrimental to the preparation.

‡ Of the specific gravity of about 1.3.

§ Of the specific gravity of about .849.

any



any convenient glass vessel, a moderate heat is to be applied until an effervescence is excited. A white fume then begins to undulate on the surface of the liquor; and the powder will be gradually precipitated, upon the cessation of action and re-action. The precipitate is to be immediately collected on a filter, well washed with distilled water, and carefully dried in a heat not much exceeding that of a water bath. The immediateedulcoration of the powder is material, because it is liable to the re-action of the nitric acid; and, whilst any of that acid adheres to it, it is very subject to the influence of light. Let it also be cautiously remembered, that the mercurial solution is to be poured *upon* the alcohol.

I have recommended quicksilver to be used in preference to an oxide, because it seems to answer equally, and is less expensive; otherwise, not only the pure red oxide, but the red nitrous oxide, and turpeth, may be substituted; neither does it seem essential to attend to the precise specific gravity of the acid, or the alcohol. The rectified spirit of wine and the nitrous acid of commerce, never failed, with me, to produce a fulminating mercury. It is indeed true, that the powder prepared without attention, is produced in different quantities, varies in colour, and probably in strength. From analogy, I am disposed to think the whitest is the strongest; for it is well known, that black precipitates of mercury approach the nearest to the metallic state. The variation in quantity is remarkable; the smallest quantity I ever obtained from 100 grains of quicksilver being 120 grains, and the largest 132 grains. Much depends on very minute circumstances. The greatest product seems to be obtained, when a vessel is used which condenses and causes most ether to return into the mother liquor; besides which, care is to be had in applying the requisite heat, that a speedy, and not a violent action be effected. 100 grains of an oxide are not so productive as 100 grains of quicksilver.

As to the colour, it seems to incline to black, when the action of the acid on the alcohol is most violent, and *vice versa*.

## SECTION X.

I need not observe, that the gases which were generated during the combustion of the powder in the glass globe, were necessarily mixed with atmospheric air; the facility with which the electric fluid passes through a vacuum, made such a mixture unavoidable.

The cubical inch of gas received over water was not readily absorbed by it: and, as it soon extinguished a taper, without becoming red, or being itself inflamed, barytes water was let up to the three cubical inches received over mercury, when a carbonate of barytes was immediately precipitated.

The residue of several explosions, after the carbonic acid had been separated, was found, by the test of nitrous gas, to contain nitrogen or azotic gas; which does not proceed from any decomposition of atmospheric air, because the powder may be made to explode under the exhausted receiver of an air-pump. It is therefore manifest, that the gases generated during the combustion of the fulminating mercury, consist of carbonic acid and nitrogen gases.

## SECTION XI.

The principal re-agents which decompose the mercurial powder, are the nitric, the sulphuric, and the muriatic acids. The nitric changes the whole into nitrous gas, carbonic acid gas, acetous acid, and nitrate of mercury. I resolved it into these different principles, by distilling it pneumatically with nitric acid: this acid, upon the application of heat, soon dissolved the powder, and extricated a quantity of gas, which was found, by well known tests, to be nitrous gas mixed with carbonic acid gas. The distillation was carried on until gas no longer came over. The liquor of the retort was then mixed with the liquor collected in the receiver, and the whole saturated with pot-ash; which precipitated the mercury in a yellowish-brown powder, nearly as it would have done from a solution of nitrate of mercury. This precipitate was separated by a filter, and the filtrated liquor evaporated to a dry salt, which was washed with alcohol. A portion of the salt being refused by this menstruum, it was separated by filtration, and recognized, by all its properties, to be nitrate of pot-ash. The alcoholic liquor was likewise evaporated to a dry salt, which, upon the affusion of a little concentrate sulphuric acid, emitted acetous acid, contaminated with a feeble smell of nitrous acid, owing to the solubility of a small portion of the nitre in the alcohol.

## SECTION XII.

The sulphuric acid acts upon the powder in a remarkable manner, as already has been noticed. A very concentrate acid produces an explosion nearly at the instant of contact, on account, I presume, of the sudden and copious disengagement of caloric from a portion of the powder which is decomposed by the acid. An acid somewhat less concentrate likewise extricates a considerable quantity of caloric, with a good deal of gas; but, as it effects a complete decomposition, it causes no explosion. An acid diluted with an equal quantity of water, by the aid of a little heat, separates the gas so much less rapidly, that it may with safety be collected in a pneumatic apparatus. But, whatever be the density of the acid, (provided no explosion be produced,) there remains in the sulphuric liquor, after the separation of the gas, a white unflammable and uncrystallized powder, mixed with some minute globules of quicksilver.

To estimate the quantity, and observe the nature, of this unflammable substance, I treated 100 grains of the fulminating mercury with sulphuric acid a little diluted. The gas being separated, I decanted off the liquor as it became clear, and freed the insoluble powder from acid, by edulcoration with distilled water; after which, I dried it, and found it weighed only 84 grains; consequently had lost 16 grains of its original weight. Suspecting, from the operation of the nitric acid in the former experiment, that these 84 grains (with the exception of the quicksilver globules) were oxalate of mercury, I digested them in nitrate of lime, and found my suspicion just. The mercury of the oxalate united to the nitric acid, and the oxalic acid to the lime. A new insoluble compound was formed;



it weighed, when washed and dry, 48,5 grains. Carbonate of pot-ash separated the lime, and formed oxalate of pot-ash, capable of precipitating lime-water, and muriate of lime; although it had been depurated from excess of alkali, and from carbonic acid, by a previous addition of acetous acid. That the mercury of the oxalate in the 84 grains, had united to the nitric acid of the nitrate of lime, was proved by dropping muriatic acid into the liquor from which the substance demonstrated to be oxalate of lime had been separated; for a copious precipitation of calomel instantly ensued.

The sulphuric liquor, decanted from the oxalate of mercury, was now added to that with which it was edulcorated, and the whole saturated with carbonate of pot-ash. As effervescence ceased, a cloudiness and precipitation followed; and the precipitate, being collected, washed, and dried, weighed 3,4 grains: it appeared to be a carbonate of mercury. Upon evaporating a portion of the saturated sulphuric liquor, I found nothing but sulphate of pot-ash; nor had it any metallic taste. There then remains, without allowing for the weight of the carbonic acid united to the 3,4 grains, a deficit from the 100 grains of mercurial powder, of 12,6 grains, which I ascribe to the gas separated by the action of the sulphuric acid. To ascertain the quantity, and examine the nature, of the gas so separated, I introduced into a very small tubulated retort, 50 grains of the mercurial powder, and poured upon it 3 drams, by measure, of sulphuric acid, diluted with an equal quantity of water, and extricated the gas with the assistance of a gentle heat. I first received it over quicksilver, the surface of which, during the operation, partially covered itself with a little black powder\*.

The gas, by different trials, amounted from 28 to 31 cubical inches; it at first appeared to be nothing but carbonic acid, as it precipitates barytes water, and extinguished a taper, without being itself inflamed, or becoming red. But, upon letting up to it liquid caustic ammoniac, there was a residue of from 5 to 7 inches of a peculiar inflammable gas, which burnt with a greenish blue flame. When I made use of the water-tub, I obtained, from the same materials, from 25 to 27 inches only of gas, although the average quantity of the peculiar inflammable gas was likewise from 5 to 7 inches; therefore, the difference of the aggregate product, over the two fluids, must have arisen from the absorption, by the water, of a part of the carbonic acid in its nascent state. The variation of the quantity of the inflammable gas, when powder from the same parcel is used, seems to depend upon the acid being a little more or less dilute.

With respect to the nature of the peculiar inflammable gas, it is plain to me, from the reasons I shall immediately adduce, that it is no other than the gas (in a pure state) into which the nitrous etherized gas can be resolved, by treatment with dilute sulphuric acid.

The Dutch chemists have shewn†, that the nitrous etherized gas can be resolved into nitrous gas, by exposure to concentrate sulphuric acid, and that, by using a dilute instead

\* I cannot account for this appearance.

† *Journal de Physique*, p. 250, October, 1794.

of a concentrate acid, a gas is obtained which enlarges the flame of a burning taper, so much like the gaseous oxide of azote, that they mistook it for that substance, until they discovered that it was permanent over water, refused to detonate with hydrogen, and that the fallacious appearance was owing to a mixture of nitrous gas with an inflammable gas.

The inflammable gas separated from the powder, answers to the description of the gas which at first deceived the Dutch chemists; 1st, in being permanent over water; 2dly, refusing to detonate with hydrogen; and, 3dly, having the appearance of the gaseous oxide of azote, when mixed with nitrous gas.

The gas separable by the same acid, from nitrous etherized gas, and from the mercurial powder, have therefore the same properties. Every chemist would thence conclude, that the nitrous etherized gas is a constituent part of the powder, had the inflammable and nitrous gas, instead of the inflammable and carbonic acid gas, been the mixed product extricated from it by dilute sulphuric acid.

It however appears to me, that nitrous gas was really produced by the action of the dilute sulphuric acid; and that, when produced, it united to an excess of oxygen present in the oxalate of mercury.

To explain how this change might happen, I must premise, that my experiments have shewn me, that oxalate of mercury can exist in two, if not in three states.

1st, By the discovery of Mr. Ameilon already quoted, the precipitate obtained by oxalic acid, from nitrate of mercury, fuses with a hissing noise. This precipitate is an oxalate of mercury, seemingly with excess of oxygen. Mercury dissolved in sulphuric acid and precipitated by oxalic acid, and also the pure red oxide of mercury digested with oxalic acid, give oxalates in the same state.

2dly, Acetate of mercury precipitated by oxalic acid, although a true oxalate is formed, has no kind of inflammability. I consider it as an oxalate with less oxygen than those above-mentioned.

3dly, A solution of nitrate of mercury boiled with dulcified spirit of nitre, gives an oxalate more inflammable than any other: perhaps it contains most oxygen.

The oxalate of mercury remaining from the powder in the sulphuric liquor, is not only always in the same state as that precipitated from acetate of mercury, entirely devoid of inflammability, but contains globules of quicksilver; consequently, it must have parted with even more than its excess of oxygen; and, if nitrous gas was present, it would of course seize at least a portion of that oxygen. It is true, that globules of quicksilver may seem incompatible with nitrous acid; but the quantity of the one may not correspond with that of the other, or the dilution of the acid may destroy its action.

As to the presence of the carbonic acid, it must have arisen either from a complete\* decomposition of a part of the oxalate; or, admitting the nitrous etherized gas to be a constituent principle of the powder, from a portion of the oxygen, not taken up by the nitrous gas, being united with the carbon of the etherized gas.

\* Inflammable oxalate of mercury, made to fuse in a retort connected with the quicksilver tub, gives out carbonic acid gas.



SECTION XIII.

The muriatic acid digested with the mercurial powder, dissolves a portion of it, without extricating any notable quantity of gas. The dissolution evaporated to a dry salt, tastes like corrosive sublimate; and the portion which the acid does not take up, is left in the state of an unflammable oxalate.

SECTION XIV.

These effects all tend to establish the existence of the nitrous etherized gas, as a constituent part of the powder; and likewise corroborate the explanation I have ventured to give, of the action of the sulphuric acid. Moreover, a measured ounce and a half of nitrous acid, holding 100 grains of mercury in solution, and 2 measured ounces of alcohol, yield 90 cubical inches only of gas: whereas, without the intervention of mercury, they yield 210 inches. Upon the whole, I trust it will be thought reasonable to conclude, that the mercurial powder is composed of the nitrous etherized gas, and of oxalate of mercury with excess of oxygen.

1st. Because the nitric acid converts the mercurial powder intirely into nitrous gas, carbonic acid gas, acctous acid, and nitrate of mercury.

2dly. Because the dilute sulphuric acid resolves it into an unflammable oxalate of mercury, and separates from it a gas resembling that into which the same acid resolves the nitrous etherized gas.

3dly. Because an unflammable oxalate is likewise left, after the muriatic acid has converted a part of it into sublimate.

4thly. Because it cannot be formed by boiling nitrate of mercury in dulcified spirit of nitre; although a very inflammable oxalate is by this means produced.

5thly. Because the difference of the product of gas, from the same measures of alcohol and nitrous acid, with and without mercury in solution, is not trifling; and,

6thly. Because nitrogen gas was generated during its combustion in the glass globe.

Should my conclusions be thought warranted by the reasons I have adduced, the theory of the combustion of the mercurial powder will be obvious to every chemist. The hydrogen of the oxalic acid, and of the etherized gas, is first united to the oxygen of the oxalate, forming water\*; the carbon is saturated with oxygen, forming carbonic acid gas; and a part, if not the whole of the nitrogen of the etherized gas, is separated in the state of nitrogen gas; both of which last gases, it may be recollected, were after the explosion present in the glass globe. The mercury is revived, and, I presume, thrown into vapour; as may well be imagined, from the immense quantity of caloric extricated, by adding concentrate sulphuric acid to the mercurial powder.

\* Drops of water were observed on the internal surface of the globe, the day after several explosions had been produced in its centre.

I will not venture to state with accuracy, in what proportions its constituent principles are combined. The affinities I have brought into play are complicated, and the constitution of the substances I have to deal with not fully known. But, to make round numbers, I will resume the statement, that 100 grains of the mercurial powder lost 16 grains of its original weight, by treatment with dilute sulphuric acid: 84 grains of mercurial oxalate, mixed with a few minute globules of quicksilver, remained undissolved in the acid. The sulphuric liquor was saturated with carbonate of pot-ash, and yielded 3,4 grains of carbonate of mercury. If 1,4 grain should be thought a proper allowance for the weight of carbonic acid in the 3,4 grains, I will make that deduction, and add the remaining 2 grains to the 84 grains of mercurial oxalate and quicksilver; I shall then have,

Of oxalate and mercury	-	-	-	-	86 grains
and a deficit, to be ascribed to the nitrous etherized gas and					
excess of oxygen	-	-	-	-	14
					<hr/> 100

It may perhaps be proper to proceed still further, and recur to the 48,5 grains, separated by nitrate of lime from the 84 grains of mercurial oxalate and globules of quicksilver, in the 11th section. These 48,5 grains were proved to be chiefly oxalate of lime; but they likewise contained a minute inseparable quantity of mercury, almost in the state of quicksilver, formerly part of the 84 grains from which they were separated. Had the 48,5 grains been pure calcareous oxalate, the quantity of pure oxalic acid in them would, according to Bergmann\*, be 23,28 grains. Hence, by omitting the 2 grains of mercury in the 3,4 grains of carbonate, 100 grains of the mercurial powder might have been said to contain, of pure oxalic acid 23,28 grains; of mercury 62,72 grains; and of nitrous etherized gas and excess of oxygen 14 grains. But, as the 48,5 grains were not pure oxalate, inasmuch as they contained the mercury they received from the 84 grains, from which they were generated by the nitrate of lime, some allowance must be made for the mercury successively intermixed with the 84 grains and the 48,5 grains.

In order to make corresponding numbers, and allow for unavoidable errors, I shall estimate the quantity of that mercury to have amounted to 2 grains, which I must of course deduct from the 23,28 grains of oxalic acid. I shall then have the following statement:

That 100 grains of the fulminating mercury ought to contain,					
of pure oxalic acid	-	-	-	-	23,28 grains,
of mercury formerly united to the oxalic acid	-	-	-	-	60,72
of mercury dissolved in the sulphuric liquor	-	-	-	-	2
and of mercury left in the sulphuric liquor after the separation of					
the gases	-	-	-	-	2
Total of mercury	-	-	-	-	<hr/> 64,72
Of nitrous etherized gas and excess of oxygen	-	-	-	-	14,
					<hr/> 100.

\* Bergmann, *de Acido Sacchari*. Opuscula. tom. i. § 6. p. 248. Leipzig, 1788.



Since 100 grains of the powder seem to contain 64,72 grains of mercury, it will be immediately inquired, what becomes of 100 grains of quicksilver, when treated as directed, in the description of the process for preparing the fulminating mercury.

It has been stated (in section 9) that 100 grains of quicksilver produce, under different circumstances, from 120 to 132 grains of mercurial powder; and, if 100 grains of this powder contain 64,72 grains, 120 grains, or 132 grains must, by parity of reasoning, contain 78,06 grains, or 85,47 grains; therefore, 13,34 grains, or 20,75 grains, more of the 100 grains are immediately accounted for; because  $64,72 \text{ grains} + 13,34 \text{ grains} = 78,06$ , and  $64,72 \text{ grains} + 20,75 \text{ grains} = 85,47 \text{ grains}$ . The remaining deficiency of 21,94 grains, or 14,53 grains, which, with the 78,06 grains, or 85,47 grains, would complete the original 100, of quicksilver, remains partly in the liquor from which the powder is separated, and is partly volatilized in the white dense fumes, which in the beginning of this paper I compared to the *liquor fumans* of Libavius. The mercury cannot, in either instance, be obtained in a form immediately indicative of its quantity; and a series of experiments to ascertain the quantities in which many different substances can combine with mercury, is not my present object. After observing, that the mercury left in the residuary liquor can be precipitated in a very subtle dark powder, by carbonate of pot-ash, I shall content myself with examining the nature of the white fumes.

#### SECTION XV.

It is clear that these white fumes contain mercury: they may be wholly condensed in a range of Woulfe's apparatus, charged with a solution of muriate of ammoniac. When the operation is over, a white powder is seen floating with ether on the saline liquor, which, if the bottles are agitated, is entirely dissolved. After the mixture has been boiled, or for some time exposed to the atmosphere, it yields to caustic ammoniac a precipitate, in all respects similar to that which is separated by caustic ammoniac from corrosive sublimate.

I would infer from these facts, that the white dense fumes consist of mercury, or perhaps oxide of mercury, united to the nitrous etherized gas; and that, when the muriate of ammoniac containing them is exposed to the atmosphere, or is boiled, the gas separates from the mercury; and the excess of nitrous acid, which always comes over with nitrous ether, decomposes the ammoniacal muriate, and forms corrosive mercurial muriate or sublimate. This theory is corroborated, by comparing the quantity of gas estimated to be contained in the fulminating mercury, with the quantities of gas yielded from alcohol and nitrous acid, with and without mercury in solution; not to mention that more ether, as well as more gas, is produced without the intervention of mercury; and that, according to the Dutch chemists, the product of ether, is always in the inverse ratio to the product of nitrous etherized gas. Should a further proof be thought necessary, of the existence of the nitrous etherized gas in the fulminating mercury, as well as in the white dense fumes, it may be added, that if a mixture of alcohol and nitrous acid holding mercury in solution, be so dilute, and exposed to a temperature so low, that neither ether nor nitrous etherized gas

gas are produced, the fulminating mercury, or the white fumes, will never be generated: for, under such circumstances, the mercury is precipitated chiefly in the state of an inflammable oxalate. Further, when we consider the different substances formed by an union of nitrous acid and alcohol, we are so far acquainted with all, except the ether and the nitrous etherized gas, as to create a presumption, that no others are capable of volatilizing mercury, at the very low temperature in which the white fumes exist, since during some minutes they are permanent over water of 40° Fahrenheit.

## SECTION XVI.

Hitherto, as much only has been said of the gas which is separated from the mercurial powder by dilute sulphuric acid, as was necessary to identify it with that into which the same acid can resolve the nitrous etherized gas; I have further to speak of its peculiarity.\*

The characteristic properties of the inflammable gas, seem to me to be the following:

- 1st. It does not diminish in volume, either with oxygen or nitrous gas.
- 2dly. It will not explode with oxygen by the electric shock, in a close vessel.
- 3dly. It burns like hydrocarbonate, but with a blueish green flame. And,
- 4thly. It is permanent over water. (Section 12.)

It is of course either not formed, or is convertible into nitrous gas, by the concentrate nitric and muriatic acids; because, by those acids, no inflammable gas was extricated from the powder.

Should this inflammable gas prove not to be a hydrocarbonate, I shall be disposed to conclude, that it has nitrogen for its basis; indeed, I am at this moment inclined to that opinion, because I find that Dr. Priestley, during his experiments on his dephlogisticated nitrous air, once produced a gas which seems to have resembled this inflammable gas, both in the mode of burning, and in the colour of the flame.

After the termination of the common solution of iron in spirit of nitre, he used heat, and got, says he, † “such a kind of air as I had brought nitrous air to be, by exposing it to iron, or liver of sulphur; for, on the first trial, a candle burned in it with a much enlarged flame. At another time, the application of a candle to air produced in this manner, was attended with a real though not a loud explosion; and, immediately after this, a greenish coloured flame descended from the top to the bottom of the vessel in which the air was contained. In the next produce of air, from the same process, the flame descended blue and very rapid, from the top to the bottom of the vessel.”

These greenish and blue coloured flames, descending from the top to the bottom of the vessel, are precisely descriptive of the inflammable gas separated from the powder. If it

\* It must be first noticed, that it is never pure when obtained from the nitrous etherized gas; nor am I aware how it is to be purified, unless the nitrous gas could be taken from it, without being converted into nitrous acid; for, by that acid, it would probably be itself converted into nitrous gas.

† Priestley on Air, Vol. II. p. 58. Birmingham. 1790.



can be produced with certainty by the repetition of Dr. Priestley's experiments, or should it by any means be got pure from the nitrous etherized gas, my curiosity will excite me to make it the object of future research; otherwise, I must confess, I shall feel more disposed to prosecute other chemical subjects: for, having reason to think that the density of the acid made a variation in the product of this gas, and having never found that *any* acid, however dense, produced an immediate explosion, I once poured 6 drams of concentrate acid upon 50 grains of the powder. An explosion, nearly at the instant of contact, was effected: I was wounded severely, and most of my apparatus destroyed. A quantity moreover of the gas I had previously prepared, was lost by the inadvertency of a person who went into my laboratory, whilst I was confined by the consequences of this discouraging accident. But, should any one be desirous of giving the gas a further examination, I again repeat, that as far as I am enabled to judge, it may with safety be prepared, by pouring 3 drams of sulphuric acid diluted with the same quantity of water, upon 50 grains of the powder, and then applying the flame of a candle until gas begins to be extricated. The only attempt I have made to decompose it, was by exposing it to copper and ammoniac; which, during several weeks, did not effect the least alteration.

(*To be continued.*)

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#### IV.

*Account of a Series of Experiments, undertaken with the View of decomposing the Muriatic Acid. By Mr. WILLIAM HENRY\*.*

MODERN chemistry, notwithstanding its rapid advancement during the few last years, still presents to its cultivators several interesting objects, both of analytic and synthetic inquiry. Among the former, the decomposition of the muriatic and of certain other acids, holds a distinguished place; for our curiosity respecting the nature of these bodies, is strongly excited, by the influence which the discovery would have on the general doctrines of chemical science, as well as on the explanation of individual facts. The theory of the formation of acids, for example, one of the most important parts of the new system of chemistry, must be regarded as incomplete, and liable to subversion, till the individual acids now alluded to have been resolved into their constituent principles. To the best of my knowledge, however, we are not in possession of a single fact that gives the smallest insight into the constitution of the muriatic acid; and the attempts to effect its analysis, can only therefore be directed by the analogy of the decomposition of other bodies, which, from similarity of character, are arranged in the same class.

\* *Philos. Trans.* 1800, page 188.

One of the first objects, in the analysis of a compound body, should be its complete separation from all other substances, which, by their presence, may tend to introduce uncertainty into the results of the processes that are employed. But it is seldom that a simplicity so desirable can be obtained in the objects of chemical research; for, agreeably to a known law of affinity, the last portions of any substance are separated with peculiar difficulty; the force of attraction appearing to increase, as we recede from the point of saturation. In a liquid state, the muriatic acid is a totally unfit subject for analytic experiment; for, in the strongest form under which it can be procured, it still contains a large proportion of water. This watery portion, besides the complexity which it introduces into the results of experiments, prevents any combustible substance that may be applied, from acting on the truly acid part; because that class of bodies, having less difficulty in attracting oxygen from water than from the acid, will necessarily take it from the former source. The state of gas, therefore, is the only one in which the muriatic acid can become a proper object of analysis.

In the series of experiments on this gas, which I am now about to describe, I employed the electric fluid, as an agent much preferable to artificial heat. This mode of operating enables us to confine accurately the gases submitted to experiment; the phenomena that occur during the process, may be distinctly observed; and the comparison of the products, with the original gases, may be instituted with great exactness. The action of the electric fluid itself, as a decomponent, is extremely powerful; for it is capable of separating from each other, the constituent parts of water, of the nitric and sulphuric acids, of the volatile alkali, of nitrous gas, and of several other bodies, whose components are strongly united. I began, therefore, with examining attentively the effects of the electric muriatic acid gas, without admixture\*.

## SECTION I.

### *On the Effects of Electricity on Muriatic Acid Gas.*

When strong electrical shocks were passed through a portion of muriatic acid gas, confined in a glass tube over mercury, the following appearances took place. The bulk of the gas, after 20 or 30 shocks, was considerably diminished; and a white deposit appeared on the inner surface of the tube, which considerably obscured its transparency. In some instances, both the contraction and deposit were much more remarkable than in others. The gas which issued from muriate of soda, soon after the affusion of sulphuric acid, and while the charge was yet warm, exhibited these appearances in an eminent degree.

\* The gases submitted to the action of electricity, in the following experiments, were confined in straight glass tubes of various diameters, armed at the sealed end with a conductor of gold, or of platina, but generally of the latter metal. The shocks were as strong as could be given without breaking the tubes, which, notwithstanding every precaution, were often shattered by the force of the explosion. Each measure of gas is equal to the bulk occupied by a grain of mercury.



Of this gas, 307 measures were reduced, by 20 shocks, to 227, or were contracted nearly one-fourth. Gas from the same materials, after they had continued working for some hours, was diminished, by similar treatment, only about a twelfth. These effects, therefore, it seemed probable, depended in some measure on the presence of moisture; and I accordingly found, that muriatic acid gas, after more than a week's exposure to muriate of lime, brought into contact with it immediately after cooling from a state of fusion, was scarcely diminished at all; and that the deposit, though it still occurred, was less copious in quantity. This deposit was not, like corrosive sublimate, soluble in water; but had every property of the less saturated salt, calomel.

The mercury by which the muriatic acid was confined, was therefore evidently oxidated; and, to the combination of a part of the gas with the oxide thus produced, the diminution of bulk was doubtless to be ascribed. But it was uncertain from whence this oxygen was derived. It might either result from the decomposition of the acid gas, or of the water chemically combined with it. The following experiments were therefore made, to determine this point.

*Experiment 1.* Through 1457 measures of muriatic acid gas, 300 electrical shocks were passed. There remained, after the admission of water, 100 measures of permanent gas, (or not quite 7 from each hundred of the original gas,) which, on trial, appeared to be purely hydrogenous.

*Exper. 2.* Of the gas, dried by muriate of lime, 176 measures received 120 shocks. The residue of hydrogenous gas amounted to 11 measures, or rather more than 6 per cent.

These experiments, and other similar ones, made on comparative portions of muriatic acid gas, in its recent state, and after exposure to muriate of lime, convinced me that it was impossible, by this method, wholly to deprive the muriatic gas of water. The recent gas, however, when electrified in smaller quantity than in experiment 1, gave a larger proportion of hydrogenous gas; which shews, that some portion of its moisture was removed, by exposure to muriate of lime. In order, if possible, to procure the gas perfectly dry, another mode of preparing it was resorted to. Alum and common salt were first well calcined, separately, to expel their water of crystallization, and, being then mixed, were distilled together in an earthen retort. The gas proceeding from these materials, was received over dry mercury; but, though only the last portion that came over was reserved for experiment, it still, after the usual electrization, afforded a product of hydrogenous gas.

In the course of the preceding experiments, I observed that the diminution of the muriatic acid gas stopped always at a certain point, beyond which it could not be carried by continuing the shocks. Gas also, which had been thus treated, when transferred to another tube, and again electrified, did not exhibit any further deposit. It became interesting, therefore, to know, whether the production of hydrogenous gas had a similar

limitation; because, the decision of this question would go far towards ascertaining its source. If the evolved hydrogenous gas arose from the decomposition of the acid, it might be expected to be produced, as long as any acid remained undecomposed. But, if water were the origin of this gas, it would cease to be evolved, when the whole of the water contained in the gas had been resolved into its constituent principles.

*Experiments 3 and 4.* Into two separate tubes, I passed known quantities of muriatic acid gas. Through the one portion, 200 discharges were taken; and through the other, 400. On comparing the quantities of hydrogenous gas produced, it proved to bear exactly the same proportion, in each tube, to the gas originally submitted to experiment. Hence it may be inferred, that the hydrogenous gas, evolved by electrifying the muriatic acid, has its origin, not from the acid, but from the water which is intimately attached to it. The agency of the electric fluid appears also, from the following experiments, to be exerted, not only in disuniting the elements of water, but in promoting the union of the evolved oxygen with muriatic acid.

*Exper. 5.* A mixture of common air and muriatic acid gas, in the proportion of 143 of the former to 116 of the latter, was rapidly diminished by electrical shocks; 30 of which reduced the whole to 111\*. The remainder consisted of muriatic acid and azote gases, with a small proportion of oxygenous gas. The deposit formed on the tube was of the same kind as before, but much more abundant.

*Exper. 6.* The same appearances were occasioned, much more remarkably, by electrifying muriatic acid with oxygenous gas: and the contraction continued, till the mercury rose so as to touch the extremity of the platina conductor. At each explosion, a dense white cloud was seen in the tube, which soon settled on its inner surface, and was of exactly the same chemical composition as the one already described. Nitrous gas and muriatic gas, when electrified together, underwent a similar change.

In order to ascertain whether the mercury by which the gases were confined, in the above experiments, had any influence on their results, they were repeated in an instrument made, purposely for the occasion, by Mr. Cuthbertson, of London. It consisted of a glass tube, ground at each end, with the view of receiving two stoppers, each perforated with platina wire, which projected into the cavity of the tube. When the stoppers were in their places, the extremities of the wires were at the distance of about half an inch; and, by properly disposing the apparatus, electrical shocks might be passed, through any gas or mixture of gases, with the contact only of glass and platina.

*Exper. 7.* In this tube I electrified the muriatic acid gas, and then admitted to it an infusion of litmus. The sudden destruction of its colour evinced the formation of oxygenated muriatic acid. Not the smallest deposit appeared on the tube.

\* This experiment suggests an additional reason, to that already given, for the greater diminution of the first, than of the subsequent portions of the muriatic acid gas; for the former may be presumed to have been much more adulterated than the latter, with the atmospherical air of the vessels.



*Experiments 8 and 9.* The same phenomenon took place, when an infusion of litmus was brought into contact with a mixture of common air and muriatic acid, and of oxygenous gas and muriatic acid, after electrization in this instrument; oxygenated muriatic acid being produced in both cases.

The above facts prove, that the combination of oxygen with muriatic acid, in these experiments, is not occasioned by a pre-disposing affinity in the mercury to combine with oxygenated muriatic acid; but that the electric fluid serves actually as an intermedium, in combining the muriatic acid with oxygen.

From the relation of these experiments it appears, that not the smallest progress had been made by them, towards the decomposition of the muriatic acid. I resolved, therefore, to attempt its analysis, in a similar manner, with the aid of combustible gases.

## SECTION II.

### *Effects of electrifying the Muriatic Acid Gas with inflammable Substances.*

In a memoir read before the Royal Society, and inserted in their Transactions for 1797, I have shewn, that when electrical shocks are passed repeatedly through a confined portion of carbonated hydrogenous gas, the water held in solution by the gas, is decomposed by the carbon, which forms a constituent part of it; that carbonic acid is formed; and an addition made, of hydrogenous gas. Hence, the bulk of the carbonated hydrogen gas is considerably enlarged by this process; which shews, by its results, that the affinity of carbon for oxygen, is rendered much more powerful and efficient by the electric fluid. I have since found, that other oxygenated substances are decomposed, by electrifying them with carbonated hydrogen gas. Nitrous gas, for example, is speedily destroyed by this process, and carbonic acid and azotic gases are obtained.

Every attempt to decompose the muriatic acid, must be founded on the presumption that it is an oxygenated substance; and those bodies promise to be the most successful agents, that possess a strong affinity for oxygen. Now, of all known bodies, charcoal most strongly attracts oxygen; and I have, therefore, repeatedly attempted the destruction of this acid, by passing it over red-hot charcoal. But, in a series of experiments, which I made some time since, with this view, in conjunction, with Mr. Rupp, we soon found reason to be dissatisfied with the difficulty and uncertainty of this process. An immense production of hydrogenous gas took place; but it was not easy to determine whether it had its origin from real acid, or from water. Our experiments, however, though insufficient to furnish decisive proof, induced us to believe that it had the latter origin.

It next occurred to me, that the comparative affinities of the muriatic radical, whatever it may be, and of charcoal, for oxygen, would be elegantly and satisfactorily ascertained, by electrifying together the carbonated hydrogen and muriatic gases. If the muriatic acid be capable of decomposition by carbon, it might be expected to be destroyed by this process; and the exact quantity of acid decomposed, and the nature and quantity of the products,

products, would thus be easily determined. I electrified, therefore, the muriatic acid and carbonated hydrogen gases, with the most scrupulous attention to the phenomena and results. That the electric fluid might not be misapplied, in decomposing the water of the carbonated hydrogen gas, it was kept more than a week, before use, over quick lime, introduced to it while yet hot.

*Exper. 10.* Of this carbonated hydrogenous gas, 186 measures were expanded, by 130 shocks, to 211; that is the gas was increased about  $\frac{1}{5}$  its bulk.

*Exper. 11.* Of the same gas, 84 measures were mixed with 116 of muriatic acid gas, dried by muriate of lime. By 120 shocks, the mixture was a little dilated. After the admission of a drop or two of water, there remained 91 measures; *i. e.* the addition of permanent gas was 7 measures, or about as much as might have been expected from the muriatic gas alone.

*Exper. 12.* Eighty-three measures of dry carbonated hydrogenous gas, with 89 of muriatic acid gas, received 200 shocks. The permanent residue, after the admission of water, was 101 measures: the addition, therefore, amounted to 18. Of the added 18, 6 may be accounted for by the decomposition of the water of the muriatic gas, and 10 by that of the carbonated hydrogenous gas. There remain, therefore, only 2 measures that can be supposed to be produced from the muriatic acid gas; a quantity too small to afford grounds for supposing them to arise from decomposed acid.

*Exper. 13.* Dry carbonated hydrogenous gas 132 measures,

mixed with dry muriatic gas	108
	<hr style="width: 50px; margin: 0;"/> 240

by 200 shocks, expanded to	268
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Part of this gas was then transferred to another tube, and the proportion of permanent gas ascertained. Through the remainder, 150 additional shocks were passed, before the amount of the gas thus evolved was determined. In both, it bore exactly the same proportion to the original gas; which shews, that by continuing the electrization, no further effects were produced.

(*To be continued.*)

## V.

*Eudiometric Observations. By Citizen BERTHOLLET\*.*

AS the air we breath is known to be composed of oxygen and azote gas, the proportions of these two gases, and the changes that may take place in the atmosphere, have been constant objects of enquiry. But the best method of acquiring that knowledge, or the result on which the greatest reliance may be placed, is not yet determined.

\* Extracted from the Memoirs on Egypt, and inserted in the *Annales de Chimie*, tom. XXXIV. p. 73.



The property of nitrous gas to absorb oxygen gas was first made use of; but it was then thought sufficient to compare the diminutions produced in the air with which the experiment was made, and the purity of any air was considered to be proportioned to the diminution it suffered.

Inquiries were afterwards directed to ascertain the real quantity of oxygen gas that combined with the nitrous gas; in order to determine by the absorption which took place in the two gases when mixed together, the relative proportions of oxygen and azote gas contained in the air of the atmosphere.

But the nitrous gas does not always afford constant results, unless by carefully observing the same manipulations. Ingenhoufs has long since shewn when the proportion of the oxygen gas is to be determined, there is no established law by which the diminution attributed to the oxygen gas can be measured, or that of the nitrous gas which becomes concentrated with it.

I am informed by an extract from the bulletin of the Philomathic Society, that M. Humboldt has endeavoured, by some very ingenious experiments, to obviate the uncertainty which prevails with regard to the differences found in the nitrous gas; and that he proposes, as a strict method of determining by this gas the exact proportion of the oxygen gas, several corrections of the quantities; but I shall prove by experiments, upon which I am still employed with Citizen Champy the younger, that this method is founded on suppositions which are inadmissible\*.

The proof by hydrogen gas, for which we are indebted to Volta, has much precision, especially when it is made upon oxygen gas; but it requires a complicated apparatus, and the hydrogen gas may differ according to the quantity of carbon it holds in solution, which may cause the result to vary considerably. Nevertheless, this method may be considered as sufficiently exact when the different airs are merely to be compared, and the same hydrogen gas is used to make this comparison. But the same accuracy cannot be had when the absolute quantity of oxygen is to be determined. The proportions by weight, of the oxygen and hydrogen that enter into the composition of water, are known with enough precision; but the relative specific gravities of the two gases are not well enough determined; and they alter too much, according to the difference of the hydrogen gas, to admit of judging exactly of the diminution which takes place by the combustion which should be attributed to the oxygen and to the hydrogen gas, and determine by that means the quantity of oxygen gas that is contained in the air with which the experiment is made.

The liquid sulphuret of alkali offers a double advantage in giving at the same time the comparative state of the different airs which are examined, and the proportion of the oxygen gas they contain. For all the diminution must here be attributed to the oxygen,

\*. These experiments were not finished when I left Cairo, and I did not bring the notes of several results; for which reason I have been obliged to repeat them, and shall soon communicate them to the world.---B.

which

which is divided between the oxygen and nitrous, or the hydrogen gas in the preceding experiments. There is no necessity for any other corrections than those which the difference of temperature and the pressure of the atmosphere require between the instant that the experiment is began, and that when the diminution of the air is measured.

It cannot be supposed that the absorption of the oxygen would be incomplete when water sufficiently charged with sulphuret of alkali is used; for there is a great difference between the power which sulphuret exerts on the oxygen gas, and the weak action the azote in the gaseous state may employ on the oxygen. And if any diminution be found in the azote which is entirely separated by this means, by afterwards mixing it with nitrous gas, I shall shew that this diminution ought not to be attributed to the oxygen.

It must be admitted, that it cannot be said that the real volume of the azote gas is that which was obtained; because azote may hold sulphuret in solution, or very probably the sulphurated hydrogen which always exists in liquid sulphuret; and it certainly has the smell, though this smell disappears on washing it in a little water without any sensible diminution of volume, so that the difference of volume which takes place in this solution must be extremely small.

There can be no apprehension that the azote should be absorbed by the sulphuret, for if this absorption took place it would continue. But the quantity of azote remains constantly the same after the oxygen is absorbed.

It is therefore, possible to determine the proportion of oxygen which is found in any quantity of air, by means of the liquid sulphuret, with all the precision that can reasonably be expected in chemistry.

The inconvenience of this method is, that the sulphuret acts slowly, and requires longer time, and particularly a low temperature; besides it gives no certain indication when the absorption is completed, except the cessation of this diminution, which requires still more time to be ascertained.

Guyton has proposed to use the dry sulphuret, by applying the heat of a candle to an apparatus he has described. I have not tried this method, but it appears to me, that so small a portion of matter may not come into good contact with so large a volume of air, by which means the absorption of all the oxygen may be rendered uncertain. The experiment does not present any indication that proves its certainty.

I have recommended the slow combustion of phosphorus. I first place a cylinder of phosphorus fixed on a stick of glass in an upright vessel, in which the air subjected to experiment is contained under water. If the temperature of the room be high, the vessel is immersed in water, in order that the phosphorus may not melt. For the evaporation of the surface of the water keeps it at a temperature some degrees below that of the atmosphere. Thus, during the experiments that I made here, the thermometer remained at about 36° degrees of the thermometer centigrade, and the bath in which the experiments were made was at six degrees below this temperature. Immediately after the phosphorus is introduced into the air, a cloud is formed, which descends and mixes with the water.

When



When the operation is finished, this cloud is no longer to be perceived. It is luminous in the dark, and as soon as it has disappeared, the absorption no longer proceeds even during the space of several days. So that by this means a certain indication of the conclusion of the experiment is obtained; and if it be made in a narrow tube, it requires no more than two hours at the temperature I have mentioned\*.

The air with which the experiment was made, is measured in a graduated tube, and when the operation is concluded, the remaining gas is measured in the same tube with the usual precautions that the changes of temperature and pressure of the atmosphere require.

The diminution obtained by phosphorus is found to be considerably less than that obtained by the sulphuret; but the proportions are always the same, because the phosphorus being dissolved in azote, as I have proved, takes the gaseous form, as do all substances which are dissolved in a gas. The volume of azote is thus augmented, and several experiments have shewn that this increase is about a fortieth part.

The difference that appears in the diminution between the phosphorus and the sulphuret cannot be caused by the one separating the oxygen less exactly than the other; for the phosphorus acts so strongly on oxygen when it is dissolved in azote, that it is sufficient to pass the phosphorated azote through water to render it luminous, and make it burn with the oxygen it meets.

It appears to me that sulphurated hydrogen, which is soluble in the azote, is capable of precipitating most of the phosphorus. For if the phosphorated azote be placed over the sulphuret of alkali, its bulk is diminished; but the total diminution is not so great as if the air had been first placed on the sulphuret. The phosphorated azote, which has been thus diminished by sulphuret, is no longer capable of being rendered luminous by coming in contact with oxygen gas.

If phosphorus be introduced into the azote which is in contact with the sulphuret, it produces no sensible effect; but if the azote be washed by being passed through clear water, it will be sufficient to render the water immediately luminous, so that the water retains the greatest part of the sulphurated hydrogen, and gives a very small quantity of oxygen to the azote gas, which by this operation is made capable of dissolving phosphorus, and of acting on the smallest quantity of oxygen gas.

I have observed, that the azote which has been exposed with the sulphuret, does not apparently lose or acquire any thing in its bulk, when it has been passed through the water, and the combustion that takes place when phosphorus is introduced is so small, that it can scarcely be appreciated.

I, therefore, conclude, from all the preceding facts, that the method with phosphorus is at the same time the most convenient and the most accurate, and that the time is not

\* These experiments were repeated at a temperature from six to ten degrees of the thermometer centigrade, and it only required six or eight hours for the operation to be concluded. M. Humboldt mentions experiments which has lasted several days, after which the gas still reddened with the nitrous gas. He must have tried very different methods to have produced so great a difference in the results.

inconvenient; that it gives the most positive results with regard to comparing the airs, and requires only one correction of a fortieth part of the volume of the remaining gas, when the proportion of the oxygen gas is to be determined with the same accuracy as may be obtained by the sulphuret, which appears to me to be the most exact approximation that can be procured by any means known\*.

Several experiments made by Citizen Champy and myself in the laboratory of the Institute of Egypt, with the sulphuret of alkali and phosphorus, in which we applied the corrections suited to the changes of temperature, as well as for the expansion of the azote by the phosphorus, have proved, that the proportion of oxygen gas in atmospheric air in that country was at least twenty two parts out of an hundred. We have not found in a great number of experiments any difference exceeding one hundredth part; and it is more natural to attribute this small variation to the inevitable imperfections attendant on all chemical processes, than to any real change in the state of the atmosphere.

Multiplied experiments I have made at Paris by the same methods, appear to me to prove, that the proportion of oxygen is there very nearly the same as in Egypt. I must, however, remark, that I have not so accurate a recollection of the results, as to affirm a perfect equality. It is, therefore, necessary to repeat these experiments at Paris, in order to establish the comparison beyond all doubt†.

Several celebrated chemists and natural philosophers give the oxygen gas a considerably higher proportion than that which I have determined; they pretend that they have found considerable variations in the air of different places and at different times. Humboldt has recently made the proportions of oxygen gas to vary from twenty-three to twenty-nine centimes. But I have not perceived these variations at the distance of Cairo from Paris,

\* Mr. Humboldt pretends, that he has formed a ternary combination of phosphorus azote and oxygen (Ann. de Chim. 30 Thermidor, An. 6.) His opinion is founded on some experiments, in which he has observed that the phosphorus produces unequal diminutions in the atmospheric air, exposed to its action in tubes of equal size, and under the same circumstances. The difference between the two tubes was from 115 to 156. It is on this alone that he establishes the existence of oxidized phosphuret of azote. I have seen no appearance of this difference in the numerous experiments I have made at Paris and at Cairo at two different periods.

† The experiments made at Paris gave about one two-hundredth part more of residue. They coincide perfectly with those which I had formerly made. It appears to me likely that this small difference arises from the air at Cairo being very dry. At the time when I operated, the air, probably, became saturated with water, and acquired by that means a slight dilation rather than a difference between the constituent parts of the two atmospheres. But the results prove that the proportion of oxygen gas might be determined at a little above twenty-two parts out of an hundred.

Experience will easily decide between the opinion of Mr. Humboldt and mine. It is only required to examine, whether it be true that phosphorus and the liquid sulphuret produce a uniform and constant result, as I affirm. I do not speak of the vivid combustion of phosphorus, with regard to which there are other considerations to be made. When the experiment of the slow action of the phosphorus is made, the cylinder of phosphorus ought to occupy a large part of the height of the tube in which the experiment is made, in order that all the atmospheric air may be subjected to its action.



at very remote periods, and in very different climates. This difference may be attributed to the nitrous gas, which is usually made use of. It is allowed, that experiments of this kind cannot be compared, if any alteration happens in the water made use of; or in the nitrous gas; or in the dimensions of the tube; or in the manipulations. Corrections founded on suppositions, very far from being justified by experiment, are very sedulously applied, and regular methods are rejected, to which no exception has ever been made but what has been overthrown by actual observation. Perhaps they are found too simple in Europe.

And, indeed, how can it be imagined that the atmosphere, which is continually agitated by motions that suddenly remove its parts, and which renew and alter its situations, can vary considerably between one village and another. There is, however, an exception to be made for those places which are elevated above the level of the sea. The difference of specific gravity between oxygen and azote gas, which in the elastic state exert a very weak mutual action, explain that difference which has been found in their proportions.

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## VI.

*Analysis of a new Variety of Lead Ore. By RICHARD CHENEVIX, Esq. Communicated by the Author.*

THE mutual elucidation which kindred sciences receive from each other, must ever increase as the means of investigation become more accurate and extensive, and in no instance can this mutual connection and improvement be more strikingly displayed, than when the influence of newly discovered facts extends itself through the whole dominion of natural knowledge.

When Sir Isaac Newton, by comparing the refracting power of water with that of other bodies, made the bold inference that it consisted partly of inflammable matter, and that the diamond possessing that power in a still higher degree, must be ranked among combustible substances, it was little suspected that chemistry would prove this not to be the vague doubt of an imagination misled by a favourite hypothesis, but the firm assertion of genius guided by strong analogical conviction. The progress made since that period affords numerous examples of such happy coincidences; and it cannot fail to be highly satisfactory, when two sciences, tending to the same point by different means, concur in affording similar results. The Abbé Haüy, on comparison of the crystalline form of the emerald and the beril, after Monf. Vauquelin had just discovered his new earth in the latter, did not hesitate to pronounce his strong suspicion of its being a constituent part of both; and analysis proved the validity of his opinion. I am happy that I have it in my power to furnish another example of the accuracy of those principles, to which he has so particularly

devoted his attention; and to call in the aid of chemistry to shew with what certitude the penetrating eye of the able crystallographer can, from external structure, arrive at an intimate knowledge of pure mineralogical substances.

In the valuable collection of the Right Honourable Charles Greville, there are many different varieties of carbonated lead, acknowledged and classed as such from their external characters: one, however, which, as far as I can learn, does not exist elsewhere, excited some doubt in the mind of *Monf. le Comte de Bournon*, from its assuming a peculiar modification of form, not hitherto remarked. He offered it to me for chemical examination; observing at the same time, that though thoroughly convinced it was carbonated lead, he was equally sure it must be in some particular state of combination, either in the proportion of its known principles, or by containing some substance not found in the other varieties. I undertook the analysis, and to render the approximation more striking, he has favoured me with the following accurate mineralogical description:

*Mineralogical Characters, in which it chiefly differs from simple Carbonated Lead, by Monf. le Comte de Bournon.*

This variety of lead ore differs from simple carbonated lead.

1. It is much softer than carbonated lead, making no impression whatever on it, and being easily scratched and worn by rubbing against it.

2. Its specific gravity is much less, being 60.651. Many experiments on pure crystals of carbonated lead give a specific gravity of 72.357. Many authors fix it at 44.63, but they have undoubtedly taken the heavy spar for it.

3. Its fracture is glassy, and much like that of the precious stones.

4. Its form is different in many respects. Like most other natural oxides of lead, its lamellæ appear to be rectangular; but their union, which very seldom forms a perfect cube in the carbonated lead, produces that form more frequently in this ore. The various laws of decrease, which these lamellæ undergo, in order to produce that cube (which is very frequently lengthened, or flattened) as well as the different crystalline modifications, are also very different.

Sometimes the decrease is at the angles, so as to replace those of the cube or parallelipipedon by planes of greater or less extent, and perpendicular to the axis of the cube. Sometimes, if we consider the cube or parallelipipedon as a prism, we shall find that the longitudinal edges of the prism are replaced by planes perpendicular to those edges.

Very frequently, the crystal being still considered as a prism, the edges of the terminating faces are replaced by planes inclined to the faces of the prism, so as to form an angle of  $148^{\circ}$ , and another angle of  $122^{\circ}$ , with the terminating faces. The crystallization under that form has sometimes been of sufficient duration, to allow at the two extremities of the crystal a tetraedal pyramid, seldom complete, but which sometimes preserves no marks of terminating faces, but a small plane replacing the summit of the pyramid; when complete, the opposite faces join at the summit in an angle of  $64^{\circ}$ .



At other times, the crystal having undergone the preceding modification, so as to leave a great part of the cubical faces, is subject to another, which replaces each of the longitudinal prismatic edges by two planes, inclined upon the prismatic faces, with which they form an angle of  $161^{\circ} 30'$ , then a tetraedral pyramid, more or less truncated at the summit, seems placed on each of the longitudinal faces (the crystals still considered as prismatic) the faces of which, taken two by two, are differently inclined; two of them tend to meet at the summit in an angle of  $143^{\circ}$ , the two others in an angle of  $116^{\circ}$ . Such are the different modifications of this ore, to which there is nothing similar among the simple carbonated leads.

All these varieties are extremely beautiful and perfect in their crystallization; they are generally of a light straw colour, though sometimes of a perfectly colourless and clear transparent white, with a lustre far more brilliant than the simple carbonated leads.

Its topographical history I shall transcribe in the words of Mr. Greville. "About fourteen or fifteen years ago, I purchased a piece of this mineral at Matlock, in Derbyshire; the shopkeeper not knowing from what mine it came, or unwilling to inform me, I left a commission for him to send me all he could procure, and repeated my order for several years successively: I traced some specimens, which had been sold, and recovered them. The largest crystal, Dr. Darwin, of Derby, gave up to me. I supposed, from my enquiries, that it came from the district of mines intended to be unwatered by the Cromford level, as I was assured of my having no chance of any more till that level was completed; and since that period the water has continued in a degree, that many mines which were then open, are now drowned."

The crystals are upon a large faceted galena, and commonly from half an inch to an inch in length. The largest mentioned in Mr. Greville's note, is an inch and a half long, an inch broad, and half an inch thick, of the finest transparent yellow. This ore, however, does not appear to be an exclusive production of this country; for there are two specimens in Mr. Greville's collection, which are in much smaller and perfectly white crystals, which seem perfectly the same; but they are not of this country, and, as I believe, come from the Hartz; no doubt it will be found elsewhere, when the attention of naturalists shall have been more particularly directed to that object.

#### A N A L Y S I S.

One hundred parts, chosen from a crystal perfectly transparent, regular, and pure, were reduced to powder; during this operation they frequently caked, and offered an elastic resistance, not unlike muriate of ammonia in the same circumstances; they were thrown into a known weight of nitrous acid; a sudden effervescence took place, and the diminution was = 00,6, and the elastic fluid was afterwards proved to be carbonic acid. As soon as the effervescence had subsided, there remained at bottom a white powder, insoluble without the assistance of heat; but after a slight ebullition, this residuum was reduced to 00,1, which was sulphate of barytes, and quite foreign to the intimate nature of the ore. Into the dissolution was poured caustic ammonia, in preference to a fixed alkali, because

because the oxide of lead is soluble in this last: A precipitate fell down, and the remaining liquor, filtered and neutralized, was submitted to the following tests:

Nitrate of lime afforded no precipitate: the presence, therefore, of the phosphoric, arsenic, or molybdic acids, was entirely precluded.

Nitrate of barytes afforded no precipitate, therefore no sulphuric acid was present.

Nitrate of lead, a slight precipitate, but quickly soluble by the addition of water. These appearances convinced me that none of those acids, hitherto found to mineralize lead ores were to be expected in the specimen now to be examined; and the properties of the precipitate caused by nitrate of lead, agreeing nearly with those of muriate of lead, I dropped in some nitrate of silver: a very thick and abundant precipitate took place, which had all the characteristics\*, not to be mistaken, of muriate of silver. It weighed 42, which, according to Bergman, contain 7 of muriatic acid; but according to Klaproth, in his analysis of the molybdate of lead, 7 of muriatic acid saturate about 45 of oxide of lead; and again, according to Bergman, 6 of carbonic acid saturate 34 of oxide of lead, which would have left a deficit of 8. The experiment was, therefore, repeated; and being aware that ammonia could no more decompose the whole of the muriate of lead, than could any other alkali, the nitrate of silver was dropped immediately into the nitric dissolution of the ore; and upon repeating this essay twice, 48 of muriate of silver were obtained each time, containing 8 of muriatic acid; which in combination form 59 of muriate of lead; the lead afterwards submitted to various proofs, was found to be perfectly pure, and the proportions may be established as follows:

Oxide of lead	-	-	-	51	} Muriate of lead.	-	-	59
Muriatic acid	-	-	-	8				
Oxide of lead	-	-	-	34	} Carbonate of lead	-	-	40
Carbonic acid	-	-	-	6				
				<hr/>				<hr/>
				99				99

With about one of heavy spar, as before mentioned.

It does not, however, appear probable, that these two salts of lead exist separately in the ore; its perfect crystallization and transparency sufficiently indicate it to be in the state of a triple salt, or carbonated muriate of lead, combined on the above proportions, and in the following order:

Muriatic	} acids	8	} Oxide of lead	85
and				
Carbonic		6		
		<hr/>		<hr/>
		14		99

The very great scarcity of this ore prevented me from submitting it to more experiments; however, upon exposing a certain quantity of it to a red heat, a thick vapour arose,

\* I need mention no other than this; muriate is the only salt of silver which is not soluble in almost every acid, and is incontestibly the most delicate and absolute re-agent in chemistry; as well to demonstrate the presence, as to determine the quantity either of silver or muriatic acid.

which



which manifested no smell of any kind, and could not, therefore, be suspected to contain either arsenic or antimony: I suppose it to be muriate of lead, as notwithstanding its great fusibility, and its supposed fixity, it is, in fact, in some degree volatile, as I have repeatedly observed on other occasions.

No more than twenty-two parts of water are necessary to dissolve one of muriate of lead, and the dissolving power is much increased by the presence of an acid, yet we see in the case before us it could resist the action of a much greater quantity, even when aided by a considerable portion of nitric acid in excess; such examples are not rare in natural substances, compared with the slight aggregation of those which art can produce.

The existence of a natural muriate of lead has been suspected more than once. Ferber examined a specimen found at Meis and Bleystadt, in Bohemia, which he imagined to contain muriatic acid; but the Baron de Born assures, that Klaproth, upon an accurate analysis, had determined those same grey hexaedral crystals, which Ferber had procured, to be merely phosphate of lead, a variety then well known. Monf. Sage had likewise asserted as much; but the specimens which he had tried, being further submitted to investigation by Mr. Laborie, and by some members of the Royal Academy of Paris, were found to be totally void of muriatic acid, and are now universally acknowledged as pure carbonated leads.

The authorities here mentioned are too respectable for me to discuss their merits. I shall only add, that after the experiments above described, and which were performed in the presence of Monf. de Bournon, he no longer refuses his assent to this new species of lead ore, now first proved, if not discovered, but has added it to those varieties already admitted in the system of mineralogy.

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## VII.

*Experiments on the Chemical Effects of Galvanic Electricity. By Mr. WILLIAM HENRY.*

To Mr. NICHOLSON.

S I R,

*Manchester, July 20, 1800.*

IN addition to the interesting facts, respecting the chemical action of galvanism, which appeared in the last Number of your Journal, I beg leave to communicate the following experiments. The apparatus, which I employed, needs no further description than that it consisted of half crowns and similar pieces of zinc, varying in number as occasion required, with pieces of woollen cloth interposed, soaked in a saturated solution of common salt. I tried muriate of lime, as a substitute for common salt, but without any better effect.

1. To

1. To the history of the phenomena, attending the decomposition of water by this new influence, which have been already accurately described by yourself, and by Messrs. Carlisle and Cruickshank, I have little to add. If the water be confined by mercury, in a tube with a conductor sealed hermetically into one end, a production of gas only takes place, when the conductor communicates with the silver side of the apparatus; and if the order be inverted, *i. e.* if the mercury be connected with the silver end, and the conductor at the sealed extremity of the tube with the zinc end, of the pile, no gas whatever is produced, though the agitation of the surface of the mercury in the tube shews that the influence is still transmitted. On passing up a wire of any metal through the quicksilver, so that its extremity may rise into the water, gas then begins to ascend copiously from the lower wire. Does not this shew, that pointed bodies are more effectual in decomposing water than bodies with rounded surfaces?

It may be worth while to observe, that the change of the colour of litmus from blue to red, occurs in distilled water, even after long boiling. From Dr. Pearson's experiments\*, however, it appears that air is separated by electric shocks from water, which has been exposed under a receiver, exhausted by a powerful air pump.

2. Concentrated sulphuric acid was submitted to this influence, in a glass tube, furnished with two platina conductors, the open end of the tube being immersed in a cupful of the same acid. Gas was produced in great plenty, one half of which was absorbed by sulphuret of potash. The remainder was hydrogenous gas, from the decomposition of water, which even the strongest sulphuric acid necessarily contains. But as the oxygenous gas was sufficient to have saturated twice the quantity of hydrogen gas evolved, one half of the former must have had another origin than water, and may be ascribed to the decomposition of the acid. Indeed, during the passage of the electricity, a white cloud was observed to furround the wire from which the gas ascended, and which was probably difoxygenated sulphur.

3. Perfectly pure and colourless nitric acid was rapidly decomposed in a similar apparatus. The acid assumed a straw colour, and gas was obtained, consisting of oxygenous and azotic gases, in the proportion of 530 of the former to 151 of the latter.

4. From liquid muriatic acid, 424 parts of gas were evolved, of which 144 were oxygenous, and 280 hydrogenous. These gases had, doubtless, their origin from the decomposition of water. The quantity of the acid being considerable, in proportion to the gases produced, it did not exhibit the marks of having become oxygenated during the experiment.

5. A tube was prepared with platina conductors, covered with wax, and being filled with a saturated solution of oxygenated muriatic acid in water, the circuit was completed through it, between the two ends of the pile. The gas evolved was a mixture of oxygenous

\* Phil. Trans. 1797, or this Journal, vol. I.



and hydrogenous, in the proportion of 136 to 118. Now the 118 parts of hydrogen gas require 59 by measure of oxygen gas for saturation. The remaining 77 parts of oxygen gas arose from the de-oxygenation of the acid.

6. As no certain conclusions respecting the composition of the muriatic acid can be drawn from experiments, instituted on it when condensed by water, I was eager to try the effects of this new and powerful agent on the acid in a gaseous form. But it was necessary to ascertain previously, whether the galvanic influence be capable of passing through aëriform bodies. An argument to the contrary was furnished by the inefficiency of this influence in inflaming mixtures of oxygen and hydrogen gases; for when the gases evolved from decomposed water, have displaced the whole of the liquid above the extremity of the upper wire, the evolved gases do not take fire, as they do in the experiment of Dr. Deiman. Another fact, shewing the non-transmission of this influence through air, is that when a little air is let up into an inverted tube filled with water, and furnished with two conductors, so as to displace the water a little below the extremity of the upper wire, no gas ascends, though the lower wire be connected with the silver base of the pile. I found, also, that a division in a piece of tin-foil pasted on glass, even when so small as not to be discerned without a magnifier, interrupted the passage of the galvanic fluid. But this non-conducting property might possibly be peculiar to common air, and it was desirable, therefore, that the transmitting power of other gases should be ascertained.

One of the most delicate tests I know of the effect of electricity on gases is furnished by the phosphorated hydrogen gas, which, by common electrical shocks, or even sparks, is expanded with great rapidity. I exposed, therefore, a portion of this gas over mercury, in a tube furnished at one end with a platina conductor, which was connected with the silver end of the pile, while the mercury that confined the gas was connected with the zinc end. But after standing several hours, no change had ensued. In a similar manner I exposed a mixture of muriatic acid and oxygenous gases, which common electrical discharges rapidly diminish, without any contraction of bulk, or change of properties\*. Neither was any permanent gas evolved by a similar treatment of muriatic acid gas alone. The deficiency of the property of transmission through gases limits considerably the use of galvanism as a chemical agent, and has totally overturned my project of attempting, by its intervention, the analysis of muriatic acid.

7. Finding it impracticable to transmit the influence through ammoniacal gas, I exposed to its action a portion of water perfectly saturated with this alkali. The result, which has been confirmed by frequent repetitions, surprized me not a little. No oxygenous gas was produced; for the evolved gas was not diminished by sulphuret of potash, neither did it inflame on passing through it an electric spark. On firing it with oxygen

\* I was misled at first to believe that a contraction had taken place, because the sealing wax, with which the platina wires were covered, had absorbed a portion of muriatic acid gas.

gas, it was diminished very greatly, and a solution of sulphuret of potash, after having abstracted the excess of oxygen gas, left only a small bubble of azotic gas, which may be traced to the oxygen gas employed to effect the combustion.

8. A solution of caustic vegetable alkali, being exposed in a similar manner, the gas produced was pure hydrogen gas. During the process, the surface of the mercury became covered with a blackish film, especially at its edges where it was in contact with the glass tube. The black matter, however, was confined to the surface of the mercury, and was not generated at the same part of the tube at which gas was produced, viz. in contact with the platina wire. It still occurred even on using mercury which had been carefully distilled for the purpose.

The two last facts are the most curious that have occurred in the course of my experiments. In experiment 7, the volatile alkali was certainly decomposed, for had the hydrogen gas proceeded from the decomposition of water only, oxygenous gas would also have been obtained. Into what new combination does the azote, in this case, enter? It is not improbable that at the same instant both water and ammoniac are decomposed; that the hydrogen of both is converted into gas; and that the oxygen of the water, uniting with the azote of the alkali, composes nitric acid, which, combining with the ammoniac, produces nitrate of ammonia. The destruction of the vegetable alkali is not less certain. From the 8th experiment we derive a sufficient proof that it contains hydrogen. Azote is probably another of its constituents, but is prevented from appearing in the gaseous state by its union with oxygen, evolved, at the same instant, by the decomposition of a portion of water. These suggestions admit of being verified by a careful examination of the alcalis, after exposure to the influence of galvanism; and a third component of the vegetable alkali will perhaps be found in the black precipitate above described. I have not been inattentive, even hitherto, to these points; but the minuteness of the quantities submitted to experiment prevents me from speaking decisively, and I choose rather to reserve myself for the results of experiments now going on, than to incur the risk of being forced to retract a hasty assertion. In the mean time, I send you these particulars of unfinished experiments, because those who are practically engaged in investigating the properties of galvanism may derive, from a communication with each other through the medium of your Journal, some of the advantages of personal intercourse and co-operation.

I am respectfully,

SIR,

Your obedient

humble servant,

WILLIAM HENRY.

*Description*



## VIII.

*Description of a new Instrument for repeating the Examination of the Strata which have been penetrated by the common boring Instrument used in Mine Works. By A. BAILLET, Inspector of Mines, and Professor at the Mineral School at Paris\*.*

AFTER remarking the great utility of boring, in order to ascertain the existence of mineral substances, particularly coal at great depths, the author observes, that it would be of great advantage if this operation could be easily repeated or verified. "It often happens," says he, "that some doubts remain concerning the nature, the thickness, or the position, of the principal mineral strata through which the borer has passed. This uncertainty increases when there is reason to suspect the integrity of the persons employed; in which case there is but one method of removing the uncertainty, viz, to bore the same excavation a second time with a tool of a larger diameter, and endeavour to confirm the results of the first operation by observing those of the second." The slowness, the expence, and the other inconveniences of this method, may easily be imagined. To avoid these the author proposes the use of an instrument, which he calls a verifier (verificateur), which is of use to take at any required depth in the hole or rectiform, a sample of the ground which exists at that spot. This instrument, represented at fig. 1, plate 9, is composed of two principal pieces; a superior piece *a b*, in which there is a cylindrical cavity opening below; and another inferior piece *c d*, inserted in this cavity, and terminating in a cone.

These two pieces are connected together by means of two keys or pins *e e*, which pass through the upper piece, and occupy a circular groove *e f* in the lower piece.

The upper piece contains two knives *g g*, let into its thickness, and fixed at their upper extremity by a screw *b*. A gutter or groove *j*, of an helical form, is made from the lower part of each blade, for the purpose of conveying the fragments and powder scraped off by these knives into the cup or cavity of the lower piece *k*.

When the apparatus is addressed, the backs of the two knives touch each other at *l*, and the point of the cone *m* lies in the angle formed by the slopes of the two knives.

The extremity *s* of the instrument terminates in a screw, which is not represented in the present drawing, and the other extremity *r* likewise terminates in a screw; so that by means of these the instrument may be lodged in any part of an hole already bored. That is to say, a rod of any required length may be fixed below it, by means of the screw *s*, and the other rods necessary to lower it down, may be screwed on above.

\* Communicated to the Philomathic Society, and inserted in their Bulletin, No. 39, in the 8th republican year.

The author remarks, 1, that the two pieces of the instrument being connected together, it can easily be let down into any hole, and drawn up again; 2, that if the lower piece rests on a fixed support, the upper piece is capable of descending through a quantity determined by the space left in the excavation, which receives the two keys in *c c*; and that during this descent, the knives will be forced asunder by the cone of the lower piece forcing itself between their backs; 3, that the upper piece is capable of turning upon the lower, in which case the knives will cut away the ground, and the fragments will fall into the groove, and be conducted into the cup beneath; 4, lastly, that if the instrument be drawn up again, the two knives will retire into their cells, either by the effect of their own elasticity, or by that of a spring properly placed for that purpose, or by the re-action of the ground itself, which presses them as they rise.

The author afterwards enters more at large into the uses of his instrument, which the reporter of the society concisely states as being effected by adapting to the instrument a series of rods successively lowered into the cavity, and of which the length is such that the knives of the instrument shall be found at the exact height of the stratum intended to be examined when the lowest rod touches the bottom. Other rods are of course to be added above, in order to conduct and manage the instrument; in this situation of things nothing more is required than to turn the apparatus in the same manner as a borer, and when it is presumed that the knives have detached a sufficient quantity, the instrument is drawn up.

The author terminates his memoir by several essential observations. The first relates to the necessity of not leaving the upper rods to the action of their own weight, which would produce the great inconvenience of separating the knives too much at first, and endangering the upper rods by too great resistance. The rods may easily be lowered properly, and by degrees, by employing the frame and screw *a*, fig. 3, through which the upper rod passes.

The second observation relates to the different methods of adjusting the verifier to the precise height where the action is intended to take place. For this purpose a rod of the proper length may be forged, in order that it may be fixed to one or more of the common rods; or otherwise the operator may provide himself with two rods of one decimetre, one of two decimetres, and another of five decimetres, which system will give all the lengths from decimetre to decimetre, from 1 to 9, &c.

The third and last observation relates to the depth of the circular cut which the two knives will make. Under like circumstances a cavity of twice the dimensions may be made with one knife instead of two. The single knife must have the form represented in fig. 2.

*General*



## IX.

*General Principles and Construction of a Sub-marine Vessel, communicated by D. BUSHNELL, of Connecticut, the Inventor, in a Letter of October, 1787, to Thomas Jefferson, then Minister Plenipotentiary of the United States at Paris.\**

THE external shape of the sub-marine vessel bore some resemblance to two upper tortoise shells of equal size, joined together; the place of entrance into the vessel being represented by the opening made by the swell of the shells, at the head of the animal. The inside was capable of containing the operator, and air, sufficient to support him thirty minutes without receiving fresh air. At the bottom opposite to the entrance was fixed a quantity of lead for ballast. At one edge which was directly before the operator, who sat upright, was an oar for rowing forward or backward. At the other edge, was a

\* Transactions of the American Philosophical Society. IV. 303.

This is the only modern instance I am acquainted with of the pursuit of sub-marine navigation. The subject was largely and pleasantly descanted upon by Merfennus in his *Traclatus de Magnetis Proprietatibus*, and Bishop Wilkins has given a chapter at some length on the same subject, in his *Mathematical Magick*. (ed. 1648) where he affirms, that Cornelius Dreble had proved, beyond all question, that the contrivance is feasible by the experiments he made in England. The chapter of Wilkins is entertaining for a sort of visionary facility with which he removes the difficulties and enumerates the benefits of these sub-marine enterprizes. For letting out and taking in such things as the nature of the voyage may require, he recommends bags, or flexible tubes, somewhat resembling the scupper bags of ships. The progressive motion may, he observes, be produced by fins or oars, which will operate with ease when the vessel is truly equipoised, and if swiftness should not be obtained, he supposes, the observations and discoveries to be made at the bottom of the sea would abundantly recompence for that defect. The greatest difficulty, in his apprehension, would be in the necessity of renovating the air for respiration and combustion; for remedying which, besides the probability that custom may render men capable of living in air of inferior purity, he has several philosophical views and projects. The conveniencies and advantages he enumerates, are, 1. Privacy; as a man may thus go to any part of the world invisibly, without being discovered or prevented. 2. Safety; from the uncertainty of tides and tempests which vex the surface; from pirates and robbers; and from the ices which so much endanger other voyages towards the poles. 3. It may be of use to undermine and blow up a navy of enemies: 4. Or to relieve a blockaded place: 5. And as the prospect enlarges in the mind of our author, he proceeds to contemplate the unspeakable benefit of sub-marine discoveries. Experiments on the ascent and descent of submerged bodies; the exploration of the deep caverns and passages of the waters of the ocean; observations on the nature and kinds of fishes, with the allurements, artifices, and treacheries, which may successfully be practiced during so familiar a residence in their territories; the food and oil they may afford; the probability of fresh springs for a supply of water at the bottom of the sea; the facility of recovering sub-marine treasures whether lost or naturally produced beneath the ocean; and last of all he adds, that

“All kinds of arts and manufactures may be exercised in this vessel. The observations made by it may be both written, and (if need were) printed here likewise. Several colonies may thus inhabit, having their children born and bred up without the knowledge of land, who could not chuse but be amazed with strange conceits upon the discovery of this upper world.”

rudder

rudder for steering. An aperture, at the bottom, with its valve, was designed to admit water, for the purpose of descending; and two brass forcing pumps served to eject the water within, when necessary for ascending. At the top, there was likewise an oar for ascending or descending, or continuing at any particular depth—A water gauge or barometer, determined the depth of descent, a compass directed the course, and a ventilator within, supplied the vessel with fresh air, when on the surface.

The entrance into the vessel was elliptical, and so small as barely to admit a person. This entrance was surrounded with a broad elliptical iron band, the lower edge of which was let into the wood of which the body of the vessel was made, in such a manner, as to give its utmost support to the body of the vessel against the pressure of the water. Above the upper edge of this iron band, there was a brass crown, or cover, resembling a hat with its crown and brim, which shut water tight upon the iron band: the crown was hung to the iron band with hinges so as to turn over sidewise, when opened. To make it perfectly secure when shut; it might be screwed down upon the band by the operator, or by a person without.

There were in the brass crown, three round doors, one directly in front, and one on each side, large enough to put the hand through—when open they admitted fresh air; their shutters were ground perfectly tight into their places with emery, hung with hinges and secured in their places when shut. There were likewise several small glass windows in the crown, for looking through, and for admitting light in the day time, with covers to secure them. There were two air pipes in the crown. A ventilator within drew fresh air through one of the air pipes, and discharged it into the lower part of the vessel; the fresh air introduced by the ventilator, expelled the impure light air through the other air pipe. Both air pipes were so constructed, that they shut themselves whenever the water rose near their tops, so that no water could enter through them, and opened themselves immediately after they rose above the water.

The vessel was chiefly ballasted with lead fixed to its bottom; when this was not sufficient, a quantity was placed within, more or less, according to the weight of the operator; its ballast made it so stiff, that there was no danger of oversetting. The vessel with all its appendages, and the operator, was of sufficient weight to settle it very low in the water. About two hundred pounds of the lead, at the bottom, for ballast, could be let down forty or fifty feet below the vessel; this enabled the operator to rise instantly to the surface of the water, in case of accident.

When the operator would descend, he placed his foot upon the top of a brass valve, depressing it, by which he opened a large aperture in the bottom of the vessel, through which the water entered at his pleasure; when he had admitted a sufficient quantity, he descended very gradually; if he admitted too much, he ejected as much as was necessary to obtain an equilibrium, by the two brass forcing pumps, which were placed at each hand. Whenever the vessel leaked, or he would ascend to the surface, he also made use of these forcing pumps. When the skilful operator had obtained an equilibrium, he could row upward, or  
downward



downward, or continue at any particular depth, with an oar, placed near the top of the vessel, formed upon the principle of the screw, the axis of the oar entering the vessel; by turning the oar one way he raised the vessel, by turning it the other way he depressed it.

A glass tube eighteen inches long, and one inch in diameter, standing upright, its upper end closed, and its lower end, which was open, screwed into a brass pipe, through which the external water had a passage into the glass tube, served as a water-gauge or barometer. There was a piece of cork with phosphorus on it, put into the water-gauge. When the vessel descended the water rose in the water-gauge, condensing the air within, and bearing the cork, with its phosphorus, on its surface. By the light of the phosphorus, the ascent of the water in the gauge was rendered visible, and the depth of the vessel under water ascertained by a graduated line.

An oar, formed upon the principle of the screw, was fixed in the forepart of the vessel; its axis entered the vessel, and being turned one way, rowed the vessel forward, but being turned the other way rowed it backward; it was made to be turned by the hand or foot.

A rudder, hung to the hinder part of the vessel, commanded it with the greatest ease. The rudder was made very elastic, and might be used for rowing forward. Its tiller was within the vessel, at the operator's right hand, fixed, at a right angle, on an iron rod, which passed through the side of the vessel; the rod had a crank on its outside end, which commanded the rudder, by means of a rod extending from the end of the crank to a kind of tiller, fixed upon the left hand of the rudder. Raising and depressing the first mentioned tiller turned the rudder as the case required.

A compass marked with phosphorus directed the course, both above and under the water; and a line and lead sounded the depth when necessary.

The internal shape of the vessel, in every possible section of it, verged towards an ellipsis, as near as the design would allow, but every horizontal section, although elliptical, yet as near to a circle, as could be admitted. The body of the vessel was made exceedingly strong; and to strengthen it as much as possible, a firm piece of wood was framed, parallel to the conjugate diameter, to prevent the sides from yielding to the great pressure of the incumbent water, in a deep immersion. This piece of wood was also a seat for the operator.

Every opening was well secured. The pumps had two sets of valves. The aperture at the bottom, for admitting water, was covered with a plate, perforated full of holes to receive the water, and prevent any thing from choking the passage, or stopping the valve from shutting. The brass valve might likewise be forced into its place with a screw, if necessary. The air pipes had a kind of hollow sphere, fixed round the top of each, to secure the air-pipe valves from injury: these hollow spheres were perforated full of holes for the passage of the air through the pipes: within the air-pipes were shutters to secure them, should any accident happen to the pipes, or the valves on their tops.

Wherever

Wherever the external apparatus passed through the body of the vessel, the joints were round, and formed by brass pipes, which were driven into the wood of the vessel, the holes through the pipes were very exactly made, and the iron rods, which passed through them, were turned in a lathe to fit them; the joints were also kept full of oil, to prevent rust and leaking. Particular attention was given to bring every part, necessary for performing the operations, both within and without the vessel, before the operator, and as conveniently as could be devised; so that every thing might be found in the dark, except the water-gauge and the compass, which were visible by the light of the phosphorus, and nothing required the operator to turn to the right hand, or to the left, to perform any thing necessary.

No. 2..

*Description of a magazine and its appendages, designed to be conveyed by the sub-marine vessel to the bottom of a ship.*

In the forepart of the brim of the crown of the sub-marine vessel, was a socket, and an iron tube, passing through the socket; the tube stood upright, and could slide up and down in the socket, six inches: at the top of the tube, was a wood screw (A) fixed by means of a rod, which passed through the tube, and screwed the wood screw fast upon the top of the tube: by pushing the wood screw up against the bottom of a ship, and turning it at the same time, it would enter the planks; driving would also answer the same purpose; when the wood screw was firmly fixed, it could be cast off by unscrewing the rod, which fastened it upon the top of the tube.

Behind the sub-marine vessel, was a place, above the rudder, for carrying a large powder magazine, this was made of two pieces of oak timber, large enough when hollowed out to contain one hundred and fifty pounds of powder, with the apparatus used in firing it, and was secured in its place by a screw, turned by the operator. A strong piece of rope extended from the magazine to the wood screw (A) above mentioned, and was fastened to both. When the wood screw was fixed, and to be cast off from its tube, the magazine was to be cast off likewise by unscrewing it, leaving it hanging to the wood screw; it was lighter than the water, that it might rise up against the object, to which the wood screw and itself were fastened.

Within the magazine was an apparatus, constructed to run any proposed length of time, under twelve hours; when it had run out its time, it unpinioned a strong lock resembling a gun lock, which gave fire to the powder. This apparatus was so pinioned, that it could not possibly move, till, by casting off the magazine from the vessel, it was set in motion.

The skilful operator could swim so low on the surface of the water, as to approach very near a ship, in the night, without fear of being discovered, and might, if he chose, approach the stem or stern above water, with very little danger. He could sink very quickly, keep at any depth he pleased, and row a great distance in any direction he desired, without coming to the surface, and when he rose to the surface, he could soon obtain a fresh supply of air, when, if necessary, he might descend again, and pursue his course.

No.



## No. 3.

*Experiments made to prove the Nature and Use of a Sub-marine Vessel.*

The first experiment I made, was with about two ounces of gun-powder, which I exploded 4 feet under water, to prove to some of the first personages in Connecticut, that powder would take fire under water.

The second experiment was made with two pounds of powder, inclosed in a wooden bottle, and fixed under a hoghead, with a two inch oak plank between the hoghead and the powder; the hoghead was loaded with stones as deep as it could swim; a wooden pipe descending through the lower head of the hoghead, and through the plank, into the powder contained in the bottle, was primed with powder. A match put to the priming, exploded the powder, which produced a very great effect, rending the plank into pieces; demolishing the hoghead; and casting the stones and the ruins of the hoghead, with a body of water, many feet into the air, to the astonishment of the spectators. This experiment was likewise made for the satisfaction of the gentlemen above-mentioned.

I afterwards made many experiments of a similar nature, some of them with large quantities of powder; they produced very violent explosions, much more than sufficient for any purpose I had in view.

In the first essays with the sub-marine vessel, I took care to prove its strength to sustain the great pressure of the incumbent water, when sunk deep, before I trusted any person to descend much below the surface: and I never suffered any person to go under water, without having a strong piece of rigging made fast to it, until I found him well acquainted with the operations necessary for his safety. After that, I made him descend and continue at particular depths, without rising or sinking, row by the compass, approach a vessel, go under her, and fix the *wood-screw* mentioned in No. 2, and marked A, into her bottom, &c. until I thought him sufficiently expert to put my design into execution.

I found, agreeably to my expectations, that it required many trials to make a person of common ingenuity, a skilful operator: the first I employed, was very ingenious, and made himself master of the business, but was taken sick in the campaign of 1776, at New-York, before he had an opportunity to make use of his skill, and never recovered his health sufficiently, afterwards.

*Experiments made with a Sub-marine Vessel.*

After various attempts to find an operator to my wish, I sent one who appeared more expert than the rest, from New-York, to a 50 gun ship lying not far from Governor's Island. He went under the ship, and attempted to fix the wooden screw into her bottom, but struck, as he supposes, a bar of iron, which passes from the rudder hinge, and is spiked under the ship's quarter. Had he moved a few inches, which he might have done, without rowing, I have no doubt but he would have found wood where he might have

fixed the screw; or if the ship were sheathed with copper, he might easily have pierced it: but not being well skilled in the management of the vessel, in attempting to move to another place, he lost the ship: after seeking her in vain, for some time, he rowed some distance, and rose to the surface of the water, but found day light had advanced so far, that he durst not renew the attempt. He says that he could easily have fastened the magazine under the stem of the ship, above water, as he rowed up to the stern, and touched it before he descended. Had he fastened it there, the explosion of one hundred and fifty pounds of powder, (the quantity contained in the magazine), must have been fatal to the ship. In his return from the ship to New-York, he passed near Governor's Island, and thought he was discovered by the enemy, on the island; being in haste to avoid the danger he feared, he cast off the magazine, as he imagined it retarded him in the swell, which was very considerable. After the magazine had been cast off one hour, the time the internal apparatus was set to run, it blew up with great violence.

Afterwards, there were two attempts made in Hudson's river, above the city, but they effected nothing. One of them was by the afore-mentioned person. In going towards the ship, he lost sight of her, and went a great distance beyond her: when he at length found her, the tide ran so strong, that as he descended under water, for the ship's bottom—it swept him away. Soon after this, the enemy went up the river, and pursued the boat which had the sub-marine vessel on board—and sunk it with their shot. Though I afterwards recovered the vessel, I found it impossible, at that time, to prosecute the design any farther. I had been in a bad state of health, from the beginning of my undertaking, and was now very unwell; the situation of public affairs was such, that I despaired of obtaining the public attention, and the assistance necessary. I was unable to support myself, and the persons I must have employed, had I proceeded. Besides, I found it absolutely necessary, that the operators should acquire more skill in the management of the vessel, before I could expect success; which would have taken up some time, and made no small additional expence. I therefore gave over the pursuit for that time, and waited for a more favorable opportunity, which never arrived.

*Other Experiments made with a Design to fire Shipping.*

In the year 1777, I made an attempt from a whale-boat, against the Cerberus frigate, then lying at anchor between Connecticut river and New London, by drawing a machine against her side, by means of a line. The machine was loaded with powder, to be exploded by a gun-lock, which was to be unpinioned by an apparatus, to be turned by being brought along side of the frigate. This machine fell in with a schooner at anchor, astern of the frigate, and concealed from my sight. By some means or other, it was fired, and demolished the schooner and three men—and blew the only one left alive, overboard, who was taken up very much hurt.

After this, I fixed several kegs, under water, charged with powder, to explode upon touching any thing, as they floated along with the tide: I set them afloat in the Delaware,



above the English shipping at Philadelphia, in December, 1777. I was unacquainted with the river, and obliged to depend upon a gentleman very imperfectly acquainted with that part of it, as I afterwards found. We went as near the shipping as he durst venture; I believe the darkness of the night greatly deceived him, as it did me. We set them adrift, to fall with the ebb, upon the shipping. Had we been within sixty rods, I believe they must have fallen in with them immediately, as I designed; but as I afterwards found, they were set adrift much too far distant, and did not arrive, until after being detained some time by frost, they advanced in the day time, in a dispersed situation, and under great disadvantages. One of them blew up a boat, with several persons in it, who imprudently handled it too freely, and thus gave the British that alarm, which brought on *the battle of the clouds*.

The above vessel, magazine, &c. were projected in the year 1771, but not completed, until the year 1775.

D. BUSHNELL.

X.

*Question respecting the Purification of Copper for alloying Gold. By X. Y.*

To Mr. NICHOLSON.

S I R,

*Newcastle-upon-Tyne, Feb. 18, 1800.*

I BEG leave, through the medium of your valuable publication, to request the favour of any of your correspondents to inform me of the best method of obtaining copper in its utmost state of purity, and fit for alloying gold without occasioning that brittleness which always arises from using copper, even of the finest kind usually obtained in commerce. The ductility of the gold may be restored by subsequent fusions, but this occasions considerable loss and disappointment to the workman, which do not happen when the copper is perfectly refined. I have looked into several chemical works, but without obtaining the satisfaction I want; nor would I be so solicitous about this information, could I, at any time, procure copper fit for this purpose.

I am, SIR,

Your's, &c.

X. Y.

## XI.

*On the Art of Hat making. Supplementary Letter. By N. L.*

To Mr. NICHOLSON.

S I R,

*Newcastle, Feb. 14, 1800.*

I SHALL think myself particularly obliged to you if you will allow me, through the medium of your Journal, to correct *a very great error in the arrangement* of the last paper of mine you printed, in your Number for May last: as it is *placed* at present, it may be productive of great mistakes, and as it will take up little room in your Journal, the inconvenience will not be great. The following is the proper arrangements, see parts of pages 74, 75, 76, vol. iii. The journeymen tell me, that the dregs are to hold or fill the body, whilst a little vitriol cleanses it of the dirt, &c. that may be on the rabbit or other wool; too much vitriol would make the whole that was weighed out to the journeymen work into the hats, but by the mutual action of the vitriol and the dregs, the quantity of the first being small, about a wine glassfull, the dirt and the strong hairs get purged out (the last from the shrinking in being slow, as well as their being straight; for was the lessening of the size at plank *rapid*, they would, in defiance of their straightness, get entangled, and even as it is, they are slightly so; but care is taken to get them out by rubbing the body of the hat well with the hand in a circular manner) whilst, at the same time, the dregs keep the hats plump. Another advantage attending the use of dregs, whether of beer, porter, or wine, is that as the boiling in the dregs does not draw out much of the mucilage from each hat, when they come to be stiffened the dregs form a body within the hat sufficiently strong or retentive to keep the glew from coming through amongst the nap: vitriol *alone* would purge or weaken the hats too much, consequently, half the quantity does better with the addition of dregs, and they disallow the body to be closer from its getting more work: many journeymen, however, to hurry this part of the process, use a quantity of vitriol, and open the body again by throwing in a handfull or two of oatmeal; by this means they get a great many made, though at the same time they are left quite grainy *from the want of labour*. This, in handling the dry grey hat, when made, may be in part discovered, but in part only; in wearing the effect is shining spots, as if of grease, but is, in reality, the glew lodging upon the grainy parts. This error in arrangement I had not discovered until I read the account of hat making, extracted from your Journal, in the supplement to the Encyclopedia Britannica published lately: I would first beg leave to add, that it was not *that* Encyclopedia alone that I meant to charge with giving



giving a wrong account of the business in question, but all the others, as *Perthenfis*, *Londinenfis*. It becomes necessary to mention this, from the manner in which the conductor of the supplement to the *Encyclopedia Britannica* has introduced the subject in question.

Believe me,

With great respect,

Your's, &c.

N. L.\*

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XII.

*On the Inventions of ROBERT HOOKE for regulating Time Pieces. By a Correspondent.*

To Mr. NICHOLSON.

S I R,

*London, July 15, 1800.*

IN that scarce publication of the famous Robert Hooke, entitled "A Description of Helioscopes and some other Instruments," printed in quarto, for Robert Martyn, printer to the Royal Society, 1676, I observe (besides the helioscopes by repeated reflexions of the solar light, and the universal joint at present so well known and esteemed) a postscript respecting the regulation of time pieces by a spring, of which I send you an abstract, in hopes that some of your ingenious readers will inform the world, whether any farther disclosure was ever made of the subject there hinted at; and also that some of your correspondents, who understand the universal and real character of Bishop Wilkins, will translate the communication of which I send you a faithful copy. It seems to me as if this wonderful man had been well acquainted with all that has since appeared as the particular discoveries of individuals during the last century.

The author first vindicates his title to the invention of what is now called the pendulum spring, contrived by him in 1658, and made the subject of a treaty between himself and some men of fortune, in 1660, which broke off upon their insisting that he should have no share in such improvements as might be afterwards made on his principles. They assured him that his invention would be discovered by others in six months, to which he answered, he would give the world seven years; and he remarks, that more than fourteen had elapsed without that effect taking place.

In 1664, at the earnest entreaty of a friend, the doctor read several of his first Cutlerean lectures on the subject of time pieces, and shewed the ground and reason of the applications of springs to the balance of a watch for regulating its motion, as the same time that he

\* I am sorry that this letter having been mislaid, renders it necessary to apologize to so valuable a correspondent.

explained above twenty several methods of application, and how the vibrations might be so regulated as to make their durations either all equal, or the greater slower or quicker than the less, and that in any proportion assigned. Some of these ways were applicable to lesser vibrations; others to greater, as of 2, 3, 4, 6, or what number of revolutions were desired; the models of which he produced in the lecture.

As his postscript is directed in controversy against Mr. Oldenburg, the writer of the Philosophical Transactions, and in part against M. Huyghens, who adopted the pendulum spring, and has described Hooke's circular pendulum to clocks in his book de Horologio Oscillatorio, without mentioning the inventor: it contains some matters of proof and animadversion, into which I have no present interest to enter; but conclude with quoting his own words respecting the copy I send you, begging leave to enter my humble protest against the injunction of secrecy, which a sense of undeserved injury may, perhaps, have extorted from this great man (page 30:)

"I shall conclude this tract with a short communication of the general ground of my invention for *pocket watches*, the number of particular ways being very great, which (that the true lovers of art, and they only may have the benefit of) I have set down in the universal and real character of the late reverend prelate my honoured friend Dr. John Wilkins, lord bishop of Chester, deceased. In which I could wish all things of this nature were communicated, it being a character and language so truly philosophical, and so perfectly and thoroughly methodical, that there seemeth to be nothing wanting to make it have the utmost perfection, and highest idea of any character imaginable, as well for philosophical as for common and constant use. And I have this further to desire of my reader, who will be at the pains to decipher and understand this description, that he would only make use of it for his own information, and not communicate the explication thereof to any, that hath not had the same curiosity with himself." See Plate X.

"This I do not so much to hinder the spreading of this description here delivered, as to revive, and, if possible, bring into use and practise that excellent design: it being a character and language perfectly free from all manner of ambiguity, and yet the most copious, expressive, and significative, of any thing or notion imaginable, and which recommends it most to common use, the most easy to be understood and learnt in the world\*."

I am,

SIR,

Your obliged reader,

M. M.

\* For some account of Bishop Wilkins's book see our Journal, II. 346.---N.



## SCIENTIFIC PUBLICATIONS.

*Philosophical Transactions of the Royal Society of London for the Year 1800. Part I. Quarto.* 238 pages, with a *Meteorological Journal*, occupying 26 pages more, and 8 Plates. Sold by Elmsly, London, price 15 Shillings.

**T**HIS part contains the following papers: 1. The Croonian Lecture. On the Structure and Uses of the Membrana Tympani of the Ear. By Everard Home, Esq. F.R.S. 2. On the Methods of determining from the real Probabilities of Life, the Nature of contingent Reversions, in which three Lines are involved in the Survivorship. By Wm. Morgan, Esq. F.R.S. 3. Abstract of a Register of the Barometer Thermometer and Rain at Lyndon, in Rutland, for the Year 1798. By Thomas Barker, Esq. 4. On the Power of penetrating into Space by Telescopes, with a comparative Determination of the Extent of that Power in natural Vision and in Telescopes of various Sizes and Constructions: illustrated by select Observations. By Wm. Herschell, LL.D. F.R.S. 5. A second Appendix to the improved Solution of a Problem in Physical Astronomy, inserted in the Philosophical Transactions for the Year 1798, containing some further Remarks, and improved Formulæ, for computing the Coefficients A and B, by which the Arithmetical work is considerably shortened and facilitated. By the Rev. John Hellins, B.D. F.R.S. and Vicar of Potters Perry, in Northamptonshire. 6. Account of a Peculiarity in the Distribution of the Arteries sent to the Limbs of slowly moving Animals: together with some other similar facts. By Anthony Carlisle, Esq. 7. Outlines of Experiments and Inquiries respecting Sound. By Thomas Young, M.D. F.R.S. 8. Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear, by Mr. Astley Cooper. 9. Experiments and Observations on the Light which is spontaneously emitted, with some degree of permanency, from various Bodies. By Nathaniel Hume, M.D. F.R.S. and A.S. 10. Account of a Series of Experiments undertaken with a View of decomposing the Muriatic Acid. By Mr. William Henry (see page 209 of the present Number.) 11. On a new Fulminating Mercury. By Edward Howard, Esq. F.R.S. (see Philof. Journal, page 173, of the present volume.) Appendix. Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

*Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide, or Dephlogisticated Nitrous Air, and its Respiration. By Humphry Davy, Superintendant of the Medical Pneumatic Institution. 580 Pages Octavo. London, printed for J. Johnson, and Biggs and Cottle, Bristol.*

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*The Doctrine of Phlogiston established, and that of the Composition of Water refuted. By Joseph Priestley, LL.D. F.R.S. &c. &c. 90 Pages Octavo. Northumberland, in America, printed for the Author, by A. Kennedy, 1800.*

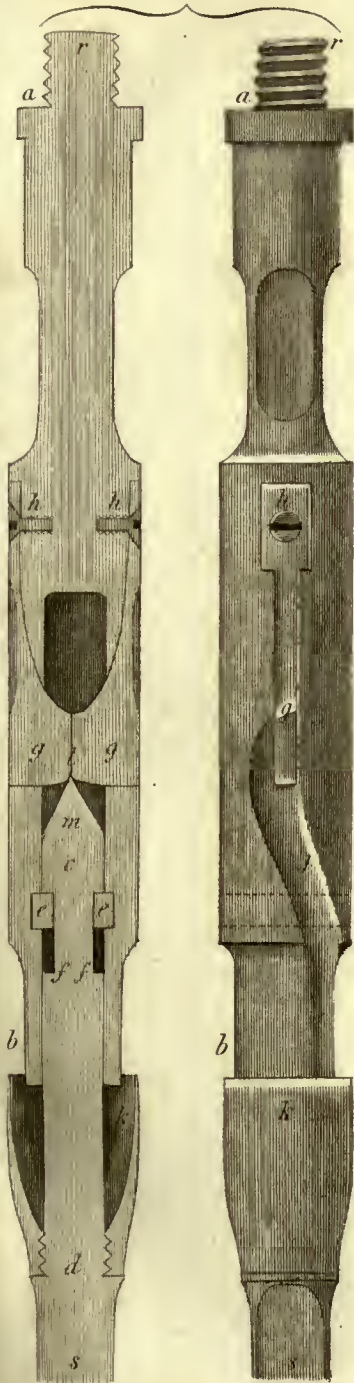
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*Transactions of the Royal Society of Edinburgh. Vol. V. Part I. for 1799. 116 Pages, Quarto.*

This work contains, 1. Investigation of certain Theorems relating to the Figure of the Earth. By Mr. Playfair. 2. Account of certain Phenomena observed in the Air Vault of the Furnaces of the Devon Iron Works: together with some practical Remarks on the Management of Blast Furnaces. By Mr. Roebuck (Philos. Jour. IV. 110.) 3. Experiments on Whinstone and Lava. By Sir James Hall, Bart. (Ph. Jour. IV. 56.) 4. A Chemical Analysis of three Species of Whinstone, and two of Lava. By Dr. Robert Kennedy. 5. A new Method of resolving Cubic Equations. By James Ivory, Esq.



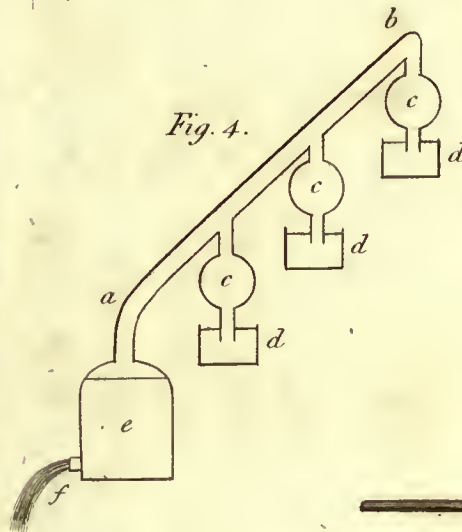
*Fig. 1.*



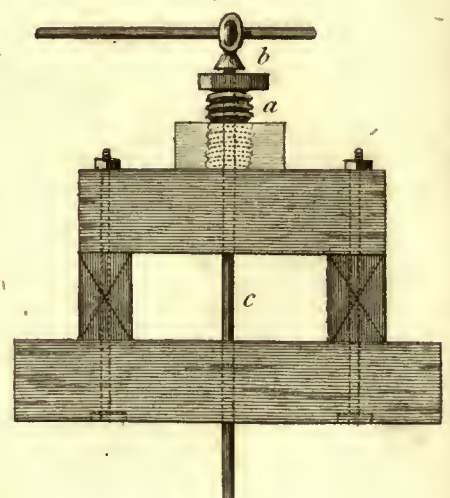
*Fig. 2.*



*Fig. 4.*



*Fig. 3.*













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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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SEPTEMBER 1800.

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ARTICLE I.

*Experiments and Observations made with the newly discovered Metallic Pile of SIG. VOLTA.  
By Lieut. Col. HENRY HALDANE. With Remarks by W. N.*

**I**MMEDIATELY after the publication of the process of Signor Volta in a morning paper \*, Col. Haldane, whose zeal in the prosecution of philosophical operation is well known, constructed an apparatus, and made a series of experiments for the purpose of analysing the pile itself, as well as the nature of its effects, of which he had the goodness to favor me with an account. But as these investigations, though undoubtedly possessing the merit of originality, as far as relates to the scientific acuteness of their author, were in some respects the same as others then unknown to him, but possessing priority in point of actual date, I postponed the public notice of his communication, till it should have the advantage of his revival, after perusing the contents of my last paper. The same motives at present lead me to give an account of the new facts, with a few remarks relative to the theory, of which we are much in want to direct our future operations.

\* Paragraph in the Morning Chronicle of May 30, 1800, giving an inaccurate account of the history of the invention, and describing the general effects as exhibited by Dr. Garnet in the lecture at the Royal Institution.

Col. Haldane's apparatus consisted of forty half crowns, with an equal number of pieces of zinc, and 39 cards, which were wetted with pure water. They were placed horizontally on a table, and the experiment of decomposing water by two copper wires, was repeated and described with minute accuracy.

He did not receive a shock with wetted hands, nor see any light when the tongue formed one of the extremes of the circuit; but his son was struck with this last appearance. Neither did his apparatus affect the electrometer of Bennet, nor my spinning instrument. When a sewing needle was passed beneath the skin of a finger of one hand, and another needle inserted in like manner in the other hand, and these were made the extremities of the circuit, a sharp irritation was felt at the wounded parts, with a convulsive sensation, extending to the shoulders, and even the neck. But this did not resemble the electric shock, being more unpleasant, and of longer duration.

The decomposition of water by silver, and by gold wires, was attended with phenomena similar to those described in our former numbers. The gold wire suffered decomposition.

In the examination of the pile, he found its power diminished by diminishing the number of pieces; that it would not act if either of the three plates were omitted; that tinfoil instead of silver acted tolerably well; and that leather was preferable to card.

When the apparatus was immersed in water, its action was intirely suspended; but upon taking it out and wiping the external surface, without separating the parts, it acted as well as before. Hence I am disposed to conclude, that the power of the Colonel's apparatus was considerably impaired by his placing them horizontally, which must have favored the efflux of water from the cards, and thence between the faces of the zinc and silver: whence his failure in the shock and electric signs. For the upright pile is certainly much stronger than a pile which has been immersed in water and then wiped.

The apparatus was suspended in the receiver of an air pump, which was then exhausted of air, till the mercury in the gage stood at half an inch. The decomposition of water in a tube with copper wires, did not proceed during this state, but was renewed when the air was let in.

This author has tried combinations of the various metals, and finds that zinc will act with gold, tin, lead, iron, and copper; that iron will act with the same metals, as will also lead, though feebly. But no other combinations of these metals would answer, except that tin and gold afforded a very faint cloud in the water. When iron and silver were used, the oxidation took place at the wire connected with the iron, and the gas flowed from that connected with the silver; but the contrary happened when zinc and iron were used, which acted very powerfully, as in this case the iron connection afforded the gas. And he considers it as worthy of remark, that the oxide of copper wire deposited in the tube was of a dusky coloured green, different from that produced by the apparatus of zinc and silver, as if some parts of the metals composing the apparatus had entered into the circulation, and affected the colour of the oxide. This observation is undoubtedly very curious, and deserves to be pursued; but I should be inclined to suppose, that the degree of oxidation:

in



in this process would vary with the intensity of the leading agent, as well as those in which the speed of the effect is governed by heat, dilution, &c.

With an increased pile placed in a vertical position, Col. H. obtained very weak signs of electricity. He connected the apparatus with the conductor of an electrical machine, and found the effect rather impeded than assisted by the common electric stream. He placed the plate of Bennet's electrometer in the circuit without producing electric signs. He found that the galvanic apparatus placed between the outside and inside of a jar prevented its charging, and that it is also capable of conducting the charge, though not rapidly: and on the whole, from the very minute exhibition of the attractive and repellent powers, while the causticity, the shock, and the oxidation, are so very powerful, he cannot be persuaded that electricity is the principal agent, though some might be generated, or disengaged during the operation of the apparatus.

Before I attempt to give a numerical elucidation of the phenomena, I shall take the liberty to remark, 1. that Col. Haldane's electric stream through the apparatus might have been in the contrary direction to its own stream; 2. that the current from an electrical machine may be incomparably less in quantity than that produced by the metals; 3. that the experiment with the cap of Bennet's electrometer was not arranged so as to direct the supposed current through the leaves; 4. that the pile must in any hypothesis prevent a jar from being charged higher than itself; 5. that Mr. Cruickshank, of Woolwich, has charged a large jar, so as to give a shock, merely by placing the pile between its coatings; and, 6. lastly, whatever may be wanting in *intensity* of electric force (upon which the signs depend) may be made up by *quantity*. Whether the quantity be, indeed, sufficient to account for what happens, must be deduced from the facts. I am aware of the great difficulties which oppose themselves to our researches into the laws by which the electric fluid is governed. All the experiments hitherto made, including a considerable number which have employed much of my time, but are still too incomplete for publication, are too few and too limited to serve as the grounds for computation, or to be extended from that intensity which affords a spark of more than a foot in length, to those minute variations of electrical power which are mensurable by sparks too short to become the object of our senses. In the following lines, therefore, when I consider the galvanic phenomena, it is simply my wish to shew that they are not inconsistent with deductions made from the present state of our knowledge, though the quantities, upon future and more strict examination, may turn out to be very different from what are here exhibited.

The apparatus of Volta may be compared with the common Leyden jar, if we extend by analogy the experiments of Cavendish to all surfaces and intensities. For shocks nearly equal will be given when the quantities of coated surface (or electrical capacity) are directly as the squares of the quantities of electricity, or inversely as the squares of the lengths of

the spark \*. But it is the character of electric shocks which are felt to equal distances from the extremities of the animal, that those which are produced by a small quantity of electricity at a high intensity, produce a sudden and more transient sensation than those which are produced by a large quantity of electricity at a low intensity, and, therefore, probably moving more slowly.

From this last circumstance I found it very difficult to take a shock from one square foot of coated glass which should much resemble the galvanic shock. When the jar was charged to give an explosive spark of one-twentieth of an inch, the shock extended above the elbows not quite so far as that from a pile of 100 half crowns; but the pain was much more sudden, sharp and transient. The dense galvanic shock seemed to fill the limbs, producing universal perspiration, and leaving a disposition to tremor and unsteadiness in the parts through which it had passed. When the explosion of the jar measured only one-fortieth of an inch, the shock still proved to be more unpleasant at the instant than that of any of the piles I have tried, though it did not extend quite to the elbows, and may be considered as equal in this respect to the set of which the electricity was tried by the condenser, as described at page 184 of our present volume. I shall, therefore, take this as one term of comparison. Another experiment was wanting to determine the length of the spark answering to the intensity at which the gold leaf of Bennet's electrometer strikes its coatings. For this purpose I took two equal electrical jars, and charged one of them, so as to give an explosive spark one-thirtieth of an inch in length. In this situation the charged jar was made to communicate with the other in the same manner as jars in a battery at top and bottom. It was then removed; having, in fact, lost half its charge. The receiving jar was in the next place discharged, and a connexion again made as before. This second process reduced the original charge to one-fourth. A third reiteration reduced it to one-eighth; and it may easily be conceived that by continuing the geometrical series, it was in my power to reduce my charge very much indeed. After seven touches the jar still caused the leaves to diverge nearly to their greatest extent, and in some repetitions of the experiment the leaves struck. The electricity in this case was reduced to  $\frac{1}{128}$  part, and, consequently, would have afforded a spark of  $\frac{1}{128}$  of  $\frac{1}{30}$ , or about  $\frac{1}{3840}$  of an inch †. We may, therefore, take it as a rule, that Bennet's electrometer, at the highest state of electricity, had this intensity.

But we are to compare this intensity with another still lower. The silk face of the condenser, used with the galvanic pile in the experiments referred to, is about one-fiftieth of an inch thick, and the leaves struck when the plate was raised about half an inch; the distance being thus increased twenty-five times. The electric intensity, or explosive spark

\* The Hon. Hen. Cavendish, Esq. found that four jars of the same size and thickness gave nearly the same, or rather a greater, shock than one of them charged with half the quantity of electricity. *Philos. Trans.* LXVI. 196.

† The length of the spark being as the quantity of electricity *ceteris paribus* in moderate intensities.



of the condenser, which was the same as that of the pile itself, was, therefore, so many times\* less than that of the electrometer; that is to say, its spark was  $\frac{1}{25} \times \frac{1}{30000} = \frac{1}{750000}$  of an inch.

Our jar of one foot surface, with its spark of  $\frac{1}{40}$  of an inch, having produced the same shock as the pile with this spark of  $\frac{1}{750000}$  of an inch, the surface of coated glass of the same thickness as the jar, and equivalent in capacity to the galvanic pile, must be as the square of  $\frac{1}{40}$  to the square of  $\frac{1}{750000}$ ; that is as 1 to  $3\frac{1}{2}$  millions nearly. Such, therefore, according to this method of computing, appears to be the capacity of the galvanic pile, so prodigiously exceeding the largest batteries ever yet constructed. But on the other hand, its intensity being so very low, the shock may be considered as if produced much more by the mass or quantity of electricity, than by its velocity of motion. Now the quantities of electricity affording equal shocks, according to the preceding conditions and doctrine, are inversely as the intensities, or in our case as 40 to 75000, or 1 to 1875, and our jar might have exploded about 10 times by each turn of a good electrical machine. The large battery of  $3\frac{1}{2}$  millions of feet would, consequently, have required 187 turns to charge it to the low galvanic intensity; and as the pile was found to charge itself in two seconds, which is very nearly the time required for each turn of a 24 inch (single) plate machine, we may assert that the production, or extrication, of electricity in a small apparatus of this kind is almost two hundred times as rapid as that which can be obtained by friction by the labour of one man.

I cannot forbear adverting again to the novelty of the field of research in which I have thus ventured to speculate. We may reasonably hope that the discoveries to which this new exhibition of the joint actions of chemistry and electricity may lead us, will shew other powers and energies of what is called the electrical fluid, and induce us to reject with gladness the imperfect theories afforded by our present knowledge of the subject †.

## II.

*Account of a Series of Experiments, undertaken with the View of decomposing the Muriatic Acid. By Mr. WILLIAM HENRY.*

(Concluded from page 214.)

A GREAT variety of similar experiments convinced me, that by electrifying together the carbonated hydrogenous and muriatic gases, not the smallest progress was made towards the decomposition of the latter. All that was thus effected, consisted in the decomposition

\* The charge at equal intensities is inversely as the thickness, as determined by Mr. Cavendish in *lococitato*. In extreme cases, however, I find that the charge follows a higher ratio.

† I have since been favoured with another communication from Col. Haldane, which I am obliged to defer to our next.

of the water of the two gases, by the carbon of the combustible gas; and, when this was completely accomplished, no further effect ensued from continuing the electrization. The generation of carbonic acid was proved, by the following experiment.

*Exper. 14.* To a mixture of carbonated hydrogen and muriatic gases, after having received above 100 shocks, a drop of water was admitted, which absorbed the muriatic acid. The liquid was then taken up by blotting-paper; and the residuary gas, being transferred into another tube, was brought into contact with a solution of pure barytic earth. The precipitation of this solution, evinced the presence of carbonic acid.

It was desirable, however, that the effects should be ascertained, of electrifying together pure muriatic acid and pure carbonated hydrogenous gas, both perfectly free from water. Now, from the experiments related in the first section, it appears highly probable, that a complete purification from moisture is produced, in both gases, by the action of the electric fluid; all the water they before contained being thus decomposed. In the following experiments, therefore, the two gases were separately electrified, before they were submitted to this process conjointly.

*Exper. 15.* To a portion of muriatic acid, diminished by the action of electricity from 144 to 121 measures, 27 measures of carbonated hydrogenous gas, expanded as far as possible, were added, and 200 shocks passed through the mixture. The addition of permanent gas amounted to 14 measures; 10 of which may be traced to the muriatic acid, and were evolved by its separate electrization. The remaining 4 measures, which remain to be accounted for, are too small a quantity to be ascribed to the decomposition of the acid.

*Exper. 16.* To a quantity of carbonated hydrogenous gas, which had received 400 shocks, and occupied the space of 212 measures, I added 232 of muriatic acid, through which 200 shocks had previously been passed. The electrization of the mixture was next continued, till 800 discharges had taken place. On examining the mixture of gases, during this operation, no change whatever took place; and, after its close, no more muriatic acid had disappeared, than would have been deficient after the first electrization; nor was there any further production of permanent gas.

*Exper. 17.* The same result was obtained, by electrifying together 280 measures of carbonated hydrogenous gas, previously expanded by 600 shocks, and 114 of muriatic acid, after 400 shocks. The additional discharge, through this mixture, of 1000 shocks, did not evince the smallest progress towards the decomposition of the muriatic acid.

*Exper. 18.* In the naturally moist state of these gases, it follows, from the 14th experiment, that carbonic acid is produced by electrifying them in conjunction. It appeared to me of some importance to ascertain whether, after a previous decomposition of their moisture, carbonic acid would continue to be generated. But the electrified carbonated hydrogenous gas itself contains carbonic acid, which, unless removed, would render the result of the experiment undecisive. This was accomplished by passing up, to a portion of electrified gas, a bubble or two of dry ammoniacal gas, which, uniting with the carbonic acid,



acid, would condense any portion of it that might be present. The remainder was transferred into another tube; and, to this carbonated hydrogenous gas, perfectly deprived both of moisture and carbonic acid, muriatic acid gas, previously electrified, was added, and electrical shocks were passed through the mixture. A drop of water was then admitted; and the residuary gas, after having been dried, was transferred into another tube. On passing up barytic water, not the smallest trace of carbonic acid could be discovered.

From the preceding experiments, the following conclusions may be deduced.

1. The muriatic acid gas, in the driest state in which it can be procured, still contains a portion of water. From a calculation founded on the experiments described in the first section, the grounds of which are too obvious to require being stated, it follows, that 100 cubical inches of muriatic gas, after exposure to muriate of lime, still hold in combination 1.4 grain of water.

2. When electrical shocks are passed through this gas, the watery portion is decomposed. The hydrogen of the water, uniting with the electric matter, constitutes hydrogenous gas, and the oxygen unites with the muriatic acid; which last, acting on the mercury, composes muriate of mercury.

3. The electric fluid serves as an intermedium, in combining oxygen with muriatic acid.

4. The really acid portion of muriatic gas does not sustain any decomposition by the action of electricity.

5. When electric shocks are passed through a mixture of carbonated hydrogen and muriatic acid gases, the water held in solution by these gases, is decomposed by the carbon of the compound inflammable gas; and carbonic acid and hydrogenous gases are the result.

6. When all the water of the two gases has been decomposed, no effect ensues from continuing the electrization; or, if the water of each gas has been previously destroyed, by electrifying them separately, no further effect ensues from electrifying them conjointly,

7. Since, therefore carbon, though placed under the most favourable circumstances for abstracting from the muriatic acid, and combining with, its oxygen, evinces no such tendency, it may be inferred, that if the muriatic acid be an oxygenated substance, its radical has a stronger affinity for oxygen than charcoal possesses.

Though the first impressions excited in my mind by the total failure of the above experiments, in accomplishing one of the greatest objects of modern chemistry, have induced me for some time to withhold them from the society, I am satisfied by reflection, that this communication is not without expediency. The means employed in attempting the analysis of the muriatic acid, were such as, after mature deliberation, appeared to me most to promise success; and the experiments were attended with a degree of labour, which can only be estimated by those who have been engaged in similar pursuits; not one third of those which were really made having been described, in the foregoing account of them.

them. It may spare therefore to others, a fruitless application of time and trouble, to be made acquainted with what I have done; and the collateral facts, which have presented themselves in the inquiry, are perhaps not without curiosity or value.

From the result of these experiments, I apprehend, all hope must be relinquished, of effecting the decomposition of the muriatic acid, in the way of single elective affinity. They furnish also a strong probability, that the basis of the muriatic acid is some unknown body; for, no combustible substance with which we are acquainted, can retain oxygen, when submitted, in contact with charcoal, to the action of electricity, or of a high temperature. The analysis of this acid must, in future, be attempted with the aid of complicated affinities. Thus, in the masterly experiment of Mr. Tennant, phosphorus, which attracts oxygen less strongly than charcoal, by the intermediation of lime decomposes the carbonic acid. Yet, led by the analogy of this fact, its discoverer found that a similar artifice did not succeed in decomposing the muriatic acid. "As vital air," he observes, "is attracted by a compound of phosphorus and calcareous earth, more powerfully than by charcoal, I was desirous of trying their efficacy upon those acids which may from analogy be supposed to contain vital air, but which are not affected by the application of charcoal. With this intention, I made phosphorus pass through a compound of marine acid and calcareous earth, and also of fluor acid and calcareous earth, but without producing in either of them any alteration. Since the strong attraction which these acids have for calcareous earth tends to prevent their decomposition, it might be thought, that in this manner they were not more disposed to part with vital air than by the attraction of charcoal: but this, however, does not appear to be the fact. I have found, that phosphorus cannot be obtained by passing marine acid through a compound of bones and charcoal when red-hot. The attraction, therefore, of phosphorus and lime for vital air, exceeds the attraction of charcoal, by a greater force than that arising from the attraction of marine acid for lime \*."

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By means similar to those employed in attempting the analysis of the muriatic acid, I tried to effect that of the fluoric acid. When electrified alone, in a glass tube coated internally with wax, it sustained a diminution of bulk, and there remained a portion of hydrogenous gas. But, neither in this mode, nor by submitting it, mixed with carbonated hydrogenous gas, to the action of electricity, was any progress made towards its analysis. These experiments, however, render it probable, that the fluoric acid, like the muriatic, is susceptible of still farther oxygenation, in which state it becomes capable of acting on mercury. The carbonic acid, on the contrary, appears not to admit of two different degrees of oxygenation. When the electric shock has been repeatedly passed through a portion of this acid gas, its bulk is enlarged, and a permanent

\* Phil. Transf. vol. LXXI. p. 184.



gas is produced, which is evidently a mixture of oxygenous and hydrogenous gases; for, when an electrical spark is passed through the gas that remains after the absorption of the carbonic acid by caustic alkali, it immediately explodes. These results even take place on electrifying carbonic acid from marble, previously calcined in a low red heat, to expel its water, and then distilled in an earthen retort \*.

## III.

*On a New Fulminating Mercury.* By EDWARD HOWARD, Esq. F. R. S.

(Concluded from page 209.)

## SECTION XVII.

I WILL now conclude, by observing, that the fulminating mercury seems to be characterised by the following properties:

It takes fire at the temperature of 368 Fahrenheit; explodes by friction †, by flint and steel, and by being thrown into concentrate sulphuric acid. It is equally inflammable under the exhausted receiver of an air-pump, as surrounded by atmospheric air; and it detonates loudly, both by the blow of a hammer, and by a strong electrical shock.

Notwithstanding the composition of fulminating silver, and of fulminating gold, differ essentially from that of fulminating mercury, all three have some similar qualities. In tremendous effects, silver undoubtedly stands first, and gold perhaps the last. The effects of the mercurial powder and of gunpowder, admit of little comparison. The one exerts, within certain limits, an almost inconceivable force: its agents seem to be gas and caloric, very suddenly set at liberty, and both mercury and water thrown into vapour. The other displays a more extended but inferior power: gas and caloric are, comparatively speaking, liberated by degrees: and water, according to Count Rumford, is thrown into vapour †.

Hence it seems, that the fulminating mercury, from the limitation of its sphere of action, can seldom if ever be applied to mining; and, from the immensity of its initial

\* Messieurs Landriani and Van Marum (*Annales de Chimie*, tom. ii. p. 270.) obtained only hydrogenous gas, by electrifying the carbonic acid gas. But the conductor of their apparatus was an iron one; which metal would combine with the oxygen of the water, and prevent it from appearing in a gaseous state. In my experiments, the conductors were of platina.

† Consequently it should not be inclosed in a bottle with a glass stopper.

‡ See Philosophical Transactions, for the year 1797, p. 222.

The hard black substance mentioned by the Count, as remaining after the combustion of gunpowder, must, I believe, have been an alkaline sulphuret, mixed chiefly with sulphite and carbonate of pot-ash. The conjecture that it is white when first formed, is certainly just, as my experiment with the glass globe evinced.

force, cannot be used in fire-arms, unless in cases where it becomes an object to destroy them; perhaps, where it is the practice to spike cannon, it may be of service, because, I apprehend, it may be used in such a manner as to burst cannon, without dispersing any splinters.

The inflammation of fulminating mercury by concussion, offers nothing more novel or remarkable, than the inflammation, by concussion, of many other substances. The theory of such inflammations has been long since exposed by the celebrated Mr. Berthollet, and confirmed by Messieurs Fourcroy and Vauquelin; yet, I must confess, I am at a loss to understand, why a small quantity of mercurial powder made to detonate by the hammer, or the electric shock, should produce a report so much louder than when it is inflamed by a match, or by flint and steel. It might at first be imagined, that the loudness of the report could be accounted for, by supposing the instant of the inflammation, and that of the powder's confinement between the hammer and anvil, to be precisely the same; but, when the electrical shock is sent through or over a few grains of the powder, merely laid on ivory, and a loud report is the consequence, I can form no idea of what causes such a report.

The operation by which the powder is prepared, is perhaps one of the most beautiful and surprising in chemistry; and it is not a little interesting to consider the affinities which are brought into play. The super-abundant nitrous acid of the mercurial solution, must first act on the alcohol, and generate ether, nitrous etherized gas, and oxalic acid. The mercury unites to the two last in their nascent state, and relinquishes fresh nitrous acid, to act upon any unaltered alcohol. The oxalic acid, although a predisposing affinity seems exerted in favour of its quantity, is evidently not formed fast enough to retain all the mercury: otherwise, no white fumes, during a considerable period of the operation, but fulminating mercury alone, would be produced.

Should any doubt still be entertained of the existence of the affinities which have been called predisposing or conspiring, a proof that such affinities really exist, will I think be afforded, by comparing the quantity of oxalic acid which can be generated from given measures of nitrous acid and alcohol, with the intervention of mercury, and the intervention of other metals. For instance, when two measured ounces of alcohol are treated with a solution of 100 grains of nickel in a measured ounce and a half of nitrous acid, little or no precipitate is produced; yet, by the addition of oxalic acid to the residuary liquor, a quantity of oxalate of nickel, after some repose, is deposited. Copper affords another illustration: 100 grains of copper, dissolved in a measured ounce and an half of nitrous acid, and treated with alcohol, yielded me about 18 grains only of oxalate; although cupreous oxalate was plentifully generated, by dropping oxalic acid into the residuary liquor. About 21 grains of pure oxalic acid seem to be produced, from the same materials, when 100 grains of mercury are interposed. (See Section 14.) Besides, according to the Dutch paper, more than once referred to, acetous acid is the principal residue after the preparation of nitrous ether. How can we explain the formation of a  
greater



greater quantity of oxalic acid, from the same materials, with the intervention of 100 grains of mercury, than with the intervention of 100 grains of copper, otherwise than by the notion of conspiring affinities, so analogous to what we see in other phenomena of nature?

I have attempted, without success, to communicate fulminating properties, by means of alcohol, to gold, platina, antimony, tin, copper, iron, lead, zinc, nickel, bismuth, cobalt, arsenic, and manganese; but I have not yet sufficiently varied my experiments, to enable me to speak with absolute certainty. Silver, when 20 grains of it were treated with nearly the same proportions of nitrous acid and alcohol as 100 grains of mercury; yielded, at the end of the operation, about 3 grains of a grey precipitate, which fulminated with extreme violence. Mr. Cruickshank had the goodness to repeat the experiment; he dissolved 40 grains of silver in 2 ounces of the strongest nitrous acid diluted with an equal quantity of water, and obtained (by means of 2 ounces of alcohol) 60 grains of a very white powder, which fulminated like the grey precipitate above described. It probably combines with the same principles as the mercury, and of course differs from Mr. Berthollet's fulminating silver, alluded to in page 250. I observe, that a white precipitate is always produced in the first instance, and that it may be preserved, by adding water, as soon as it is formed; otherwise, when the mother liquor is abundant, it often becomes grey, and is re-dissolved.

P.S. Since the preceding pages were written, I have been permitted, by the Right Honourable Lord Howe, Lieutenant General of the Ordnance, to make the following trials of the mercurial powder, at Woolwich, in conjunction with Colonel Blomefield, and Mr. Cruickshank\*.

*Experiment 1.* From the manner in which the screw of the gun-breech, mentioned in Section v. had acted on the barrel, it was imagined, that by bursting an iron case, exactly fitted to the bore of a cannon, its sudden enlargement might make many flaws, and split the piece, without dispersing any splinters. In conformity to this opinion, a cast iron case was constructed, with a cylindrical chamber, of equal length and diameter, calculated to hold  $3\frac{1}{4}$  ounces troy of the mercurial powder. The case, being firmly screwed together, was charged through its vent-hole, and introduced into a twelve-pounder carronade, the bore of which it exactly fitted. The powder was then enflamed, with proper precautions. The gun remained entire, but the case divided: the portion forming the upper surface of the chamber, was expelled in one mass; that adjoining the breech, which constituted the rest of the chamber, was cracked in every direction, and in part crumbled; yet it was so wedged into some indentations which the explosion had made in the sides of the piece, that the fragments were not removed without great labour.

*Experiment 2.* Another cast iron case was prepared, of the same size as the former, with a chamber also cylindrical, but wrought in a transverse direction, and of a greater length

\* It is with pleasure I take this opportunity of acknowledging the civil attention I received from the different officers.

than diameter; the thickness of metal at each extremity not being more than a quarter of an inch. This case was filled with nearly 5 ounces troy of the mercurial powder, and placed in the same carronade. Three twelve-pound shot were next introduced, and brought into close contact with the upper surface of the case, as well as with each other. The gun a second time withstood the explosion: the case was divided across the middle of the chamber, into two equal parts; that adjoining the breech was, as in the former experiment, much flayed, and left immovable; that nearest to the muzzle was also much flayed, but driven out with the shot. All the three shot were broken; the two lower being divided into several pieces, and the upper one cracked through the centre.

The report was so feeble, in both experiments, that an inattentive person, I am confident, would not have heard it at the distance of two hundred yards.

*Experiment 3.* It was found so difficult to extract the fragments of the case remaining in the carronade, after the last experiment, that a channel was drilled through them, to the vent-hole of the piece. It was then charged with 6 ounces troy of the mercurial powder, made up as a cartridge, which did not occupy above one half of the diameter of the bore. A wad was placed over the powder, dry sand superadded, to fill all vacuities, and the gun filled to the muzzle with two twelve-pound shot. A block of wood was set at a small distance, to receive the impression of the shot, and the powder was inflamed as usual. The carronade still resisted. One of the shot was split into two pieces; and the block of wood was driven to a considerable distance, but not penetrated by the shot above the depth of one inch. The report was somewhat louder than the former ones. In all three instances, a considerable recoil evidently took place. I presume, therefore, that in the first experiment related in the fifth Section, there must have been a recoil, though too trifling to be observed; and, in the instances where the gun and the proof were burst, it was not so much to be expected.

*Experiment 4.* Finding that the carronade, from the great comparative size of its bore to that of its length, required a larger quantity of mercurial powder to burst it than we were provided with, we charged a half-pounder swivel with an ounce and a half avoirdupois of the mercurial powder, (the service charge of gunpowder being 3 ounces) and a half-pound shot between two wads. The piece was destroyed from the trunnions to the breech, and its fragments thrown thirty or forty yards. The ball penetrated five inches into a block of wood, standing at about a yard from the muzzle of the gun; the part of the swivel not broken, was scarce, if at all, moved from its original position.

*Experiment 5.* One ounce avoirdupois of the mercurial powder, enclosed in paper, was placed in the centre of a shell 4,4 inches in diameter, and the vacant space filled with dry sand.

The shell burst by the explosion of the powder, and the fragments were thrown to a considerable distance. The charge of gunpowder employed to burst shells of this diameter, is 5 ounces avoirdupois.

*Experiment*



*Experiment 6.* A sea grenade, 3.5 inches diameter, charged like the shell in the last experiment, was burst into numerous fragments, by  $\frac{1}{4}$  of an ounce avoirdupois of the mercurial powder. The fragments were projected with but little force, and only to the distance of eight or ten yards. The charge of gunpowder required for grenades of this size, is 3 ounces.

*Experiment 7.* A sea grenade, of the same diameter as the last mentioned, and charged in the like manner, with  $\frac{1}{8}$  of an ounce avoirdupois, or  $57\frac{1}{2}$  grains, of the mercurial powder, was split into two equal pieces, which were not thrown ten inches asunder.

The report in the four last experiments was very sharp, but not loud in proportion.

It seems, from the manner in which the swivel was burst, in the fourth experiment, that a smaller charge would have been sufficient for the purpose. We may therefore infer, both from this instance and from the second experiment made with the gun, in Section v, that any piece of ordnance might be destroyed, by employing a quantity of the mercurial powder equal in weight to one half of the service charge of gunpowder; and, from the seventh and last experiment, we may also conclude, that it would be possible so to proportion the charge of mercurial powder to the size of different cannons, as to burst them without dispersing any splinters. But the great danger attending the use of fulminating mercury, on account of the facility with which it explodes, will probably prevent its being employed for that purpose.

In addition to the other singular properties of the fulminating mercury, it may be observed, that two ounces inflamed in the open air, seem to produce a report much louder than when the same quantity is exploded in a gun capable of resisting its action. Mr. Cruickshank, who made some of the powder, by my process, remarked that it would not inflame gunpowder. In consequence of which, we spread a mixture of coarse and fine grained gunpowder upon a parcel of the mercurial powder; and, after the inflammation of the latter, we collected most, if not all, of the grains of gunpowder. Can this extraordinary fact be explained by the rapidity of the combustion of fulminating mercury? or is it to be supposed, (as gunpowder will not explode at the temperature at which mercury is thrown into vapour,) that sufficient caloric is not extricated during this combustion.

From the late opportunity I have had of conversing with Mr. Cruickshank, I find that he has made many accurate experiments on gunpowder; and he has permitted me to state, "that the matter which remains after the explosion of gunpowder, consists of pot-ash, united with a small proportion of carbonic acid, sulphate of pot-ash, a very small quantity of sulphuret of pot-ash, and uncombusted charcoal. That 100 grains of good gunpowder yield about 53 grains of this residuum, of which three are charcoal. That it is extremely deliquescent, and, when exposed to the air, soon absorbs moisture sufficient to dissolve a part of the alkali; in consequence of which, the charcoal becomes exposed; and the whole assumes a black or very dark colour." Mr. Cruickshank likewise informs me, that after the combustion of good gunpowder under mercury, no water is ever perceptible.

*References to the Figures of the glass Globe, &c. mentioned in Section VII.*

A, (Pl. VII. of the present volume) a ball or globe of glass, nearly half an inch thick, and seven inches in diameter. It has two necks, on which are cemented the brass caps, B, C, each being perforated with a female screw, to receive the male ones D, E: through the former a small hole is drilled; the latter is furnished with a perforated stud or shank G. By means of a leather collar H, the neck C can be air-tightly closed. When a portion of the powder is to be exploded, it must be placed on a piece of paper, and a small wire laid across the paper, through the midst of the powder: the paper being then closed, is to be tied at each end to the wire, with a silken thread, as shewn at I. One end of this wire is to be fastened to the end of the shank G, and the screw D inserted to half its length into the brass cap B; the other end of the wire, *a*, by means of the needle K, is to be drawn through the hole F. The screw E being now fixed in its place, and the wire drawn tight, it is to be secured, by pushing the irregular wooden plug L into the aperture of the screw D, taking care to leave a passage for air. The stop-cock M, the section of which is shewn at N, is now to be screwed on to the part D, which is made air-tight by the leather collar *b*. The glass tube O is bent, that it may more conveniently be introduced under the receiver of a pneumatic apparatus. P, shews the manner of connecting the glass tube with the stop-cock.

## IV.

*Additional Remarks on Galvanic Electricity. By Mr. W. CRUICKSHANK, Woolwich.*

*Communicated by the Author.*

BEING desirous to ascertain with some degree of precision, the nature and relative proportions of the gases obtained from water and other fluids by this influence, I procured some wires of gold, and also some of silver gilt. On trial, I found that these wires were not by any means so much acted upon as silver, at least where water alone was decomposed; I likewise observed, that the quantity of oxygen in the mixed gas was much greater, and amounted to nearly one third of the whole; the gas from the zinc wire was also more copious, than when silver or copper wires were employed.

I took a wide mouthed phial capable of containing 3 ounces, and filled it with very pure lime water, a cork was then loosely introduced into its mouth, and two gold wires passed through it; the phial being inverted in a small basin containing pure water, the other extremities of the wires were connected with those of the pile in the usual way; a very copious production of gas immediately took place, more especially from the silver wire, and in about four hours the phial was completely filled. This gas was submitted to the following experiments:

One measure of it being mixed with two of nitrous gas, a diminution of one measure took place, the residuum contained nitrous gas, mixed with hydrogen.

Four



Four measures of this gas were next exploded by the electric spark in a strong glass jar over mercury, and the whole disappeared, except about  $\frac{1}{3}$  of a measure; this was not inflammable, and appeared to be azote.

From these experiments it would seem, that this compound gas obtained from water by means of gold wires, consists of nearly two parts hydrogen and one oxygen, mixed with a little azote, being nearly the proportions estimating by bulk, which are said to enter into the composition of water.

It has been supposed, although not proved by Mr. Nicholson, that the gas which escapes from the wire connected with the silver extremity of the pile is hydrogen, whilst that disengaged by the one connected with the zinc is oxygen gas.

In order to satisfy myself about the nature and proportions of these gases, I took a glass tube 10 inches in length, and by means of the blow pipe bent it in the middle, until the legs formed an acute angle resembling the letter V; while the glass was red hot, I contrived to blow an opening at the angle about  $\frac{1}{10}$  of an inch, or a little better in diameter. Two gold wires passed through corks secured by cement, were introduced into the legs, and brought within an inch of each other at the bend. The tube was then filled with distilled water, and a finger being placed on the opening at the angle, to prevent the fluid from escaping, it was placed in a tea-cup containing water, with the angle downwards, the legs having an inclination of about 45 degrees. The extremities of the wires being then brought into contact with those of the pile, a quantity of gas was disengaged from both, but by far the greatest from that connected with the silver; by this contrivance the gases from the two wires were obtained perfectly distinct, each gas ascending in the leg of the tube which contained its generating wire. When a sufficient quantity of the two aeriform fluids had been thus obtained, they were examined as follows:

One measure of the gas from the silver side, was mixed with one of nitrous gas, some red fumes were observed, and a diminution which amounted to  $\frac{1}{3}$  of a measure took place; the residuum consisted of a mixture of nitrous and hydrogen gas.

Two measures of this gas mixed with one of pure oxygen, being next introduced into a strong glass jar over mercury, and fired by the electric spark, the whole very nearly disappeared, not more than  $\frac{2}{5}$  of a measure remaining; this residuary gas appeared from the nitrous test to be chiefly oxygen. A dense white vapour was perceived over the mercury for some time after the explosion.

One measure of the gas from the zinc side being mixed with two of very pure nitrous gas, the whole very nearly disappeared, and another measure of nitrous gas being added, the total diminution amounted to nearly three measures. From these experiments it would appear, that the gas obtained from the silver wire was chiefly hydrogen gas, and that from the zinc wire, nearly pure oxygen.

Having been favoured by Mr. Nicholson with small pieces of platina wires, I contrived to fasten them to silver ones, and these last being covered with a composition of resin and bees wax, the platina alone was exposed to the action of the water, &c. By means of these

these platina points, I obtained gases very nearly similar to those just described: but the oxygen was not quite so pure, as it often contains  $\frac{1}{3}$  or  $\frac{1}{4}$  of azote; and when the mixed gases were exploded, as in the first experiment with the gold wires, there was a residuum amounting to  $\frac{1}{7}$  of the whole, which was found to be a mixture of hydrogenous and azotic gases. It may be proper to observe, that in all those cases where the gases were obtained separately, the volume of gas from the silver side was nearly three times that from the zinc side; and that the former always contained a little oxygen, amounting to about  $\frac{1}{8}$  or  $\frac{1}{10}$  of the whole\*.

The platina point, connected with the zinc as well as the gold wire on the same side, were after some time evidently tarnished; and this effect was soon observed when the machine was in full action.

In the following experiment I obtained a perfect solution of gold:

After having precipitated magnesia and argill from their solutions in acids, by the influence of the wire from the silver side, I was desirous to see what effect would be produced on solutions of lime. For this purpose the glass tube was filled with a solution of the muriate of lime which had been crystallised, and the gold wires applied in the usual manner. When the tube was placed in the circle of communication, little or no gas escaped from the silver wire, but a considerable quantity began to ascend immediately from the zinc one, and the fluid surrounding it assumed a fine yellow colour, which was found to proceed from a solution of the gold, the wire at the end of the process being much corroded. After some time gas was likewise disengaged from the silver wire, but there was not the least precipitation of lime; when the wires were removed the fluid smelled of aqua regia, or the ox. muriatic acid.

When platina points were employed instead of the gold wires, the smell of the nitro-muriatic acid was also soon perceived, but no sensible solution of the platina was observable.

This formation of the nitro-muriatic, or oxygenated muriatic acids, was not observed but where the perfect metals gold and platina were employed; the reason of which must be obvious. When the tube was filled with a solution of common salt instead of the muriate of lime, a nitro-muriatic acid was likewise produced. The effects produced by the gold or platina wires on the tinctures of Litmus and Brazil wood, were still more remarkable than those described in the former paper, more particularly on the litmus, which was very quickly reddened. When the tube was filled with distilled water only, and gold wires employed; after the influence had passed through for some time (without confining the gas) that portion of the fluid in contact with the silver wire being decanted, strongly reddened the tincture of Brazil wood. This mode of making the experiment was preferred, as it might be supposed that the hydrogen in its nascent state would unite with the colouring matter of the Brazil wood, and produce the effect of an alkali.

\* Was this oxygen originally held in solution by the water? C.



From the experiments related in this and the former paper, we may draw the following conclusions :

1. That hydrogen gas mixed with a very small proportion of oxygen and ammonia, is somehow disengaged at the wire connected with the silver extremity of the machine; and that this effect is equally produced, whatever the nature of the metallic wire may be, provided the fluid operated upon be pure water.

2. That where metallic solutions are employed instead of water, the same wire which separates the hydrogen revives the metallic calx, and deposits it at the extremity of the wire in its pure metallic state; in this case no hydrogen gas is disengaged. The wire employed for this purpose may be of any metal.

3. That of the earthy solutions, those of magnesia and argill only are decomposed by the silver wire, a circumstance which strongly favours the production of ammonia.

4. That when the wire connected with the zinc extremity of the pile consists either of gold or platina, a quantity of oxygen gas, mixed with a little azote and nitrous acid is disengaged, and the quantity of gas thus obtained is a little better than  $\frac{1}{2}$  of the hydrogen gas separated by the silver wire at the same time.

5. That when the wire connected with the zinc is silver, or any of the imperfect metals, a small portion of oxygenous gas is likewise given out, but the wire itself is either oxydated or dissolved, or partly oxydated and partly dissolved; indeed, the effect in this case produced upon the metal, is very similar to that of the concentrated nitrous acid, where a great deal of the metal is oxydated, and but a small quantity held in solution\*.

6. That when the gases obtained by gold or platina wires, are collected together and exploded over mercury, the whole nearly disappears and forms water, with probably a little nitrous acid, for there was always a thick white vapour perceived for some time after the explosion. The residuary gas in this case appeared to be azote.

In reflecting on these experiments it would appear, that in some of them, the water must be decomposed; but how this can be effected, is by no means so easily explained. For example, it seems extremely mysterious how the oxygen should pass silently from the extremity of the silver wire to that of the zinc wire, and there make its appearance in the form of gas. It is to be observed likewise, that this effect takes place which ever way the wires are placed, and whatever bends may be interposed between their extremities, provided the distance be not too great. On considering these facts more minutely, it appeared to me, that the easiest and simplest mode of explanation, would be, to suppose that the galvanic influence (whatever it may be) is capable of existing in two states, that is, in an oxygenated and deoxygenated state. That when it passes from metals to fluids containing

\* The great difference in the effect produced by this influence on gold and silver, which have always been considered as equally difficult to oxidate, can only be explained on the supposition, that nitrous acid is generated; for this acid, it is well known, acts powerfully on silver, but has no action whatever on gold. The same observation applies to platina;

oxygen, it seizes their oxygen, and becomes oxygenated; but when it passes from the fluid to the metal again, it assumes its former state, and becomes deoxygenated. Now when water is the fluid interposed, and the influence enters it from the silver side deoxygenated, (and we suppose that it always passes from the deoxygenated to the oxygenated side) it seizes the oxygen of the water, and disengages the hydrogen, which accordingly appears in the form of gas; but when the influence enters the zinc wire, it parts with the oxygen, with which it had formerly united, and this either escapes in the form of gas, unites with the metal to form an oxyde, or, combined with a certain portion of water, &c. may, according to the German chemists, form nitrous acid. When a metallic solution is the interposed fluid, the effect produced may be explained in two ways\*, but the simplest is to suppose, that the influence in passing from the silver wire, seizes the oxygen of the metallic calx, and afterwards deposits it on entering the zinc one; in this case no gas should appear at the silver wire, but when a perfect metal is employed, oxygen should be disengaged from the zinc wire; and this, as has been already mentioned, is exactly what takes place. What I consider, however, as the strongest argument in favour of this hypothesis, is, that all fluids which do not contain oxygen, are incapable of transmitting the galvanic fluid, such as alcohol, æther, the fat, and essential oils, as I have proved by direct experiment; but on the contrary, that all those which do contain oxygen conduct it more or less readily, as all aqueous fluids, metallic solutions, and acids, more especially the concentrated sulphuric acid; which it decomposes. In this last instance, the oxygen produced can hardly be ascribed to the decomposition of water; for this acid, when properly concentrated, does not contain any sensible quantity. By this theory also, we can readily explain the oxydation of the zinc plates in the machine; where the fluid in passing from the different pairs of plates, appears to be alternately oxygenated and deoxygenated. Although I am not by any means entirely satisfied with this hypothesis, yet, as it is the only one by which I can explain the different phenomena, it was thought adviseable to throw it out, merely with a view to induce others to reason upon the subject, and to incite them to make experiments, by which alone truth can be ascertained.

A convenient and powerful machine is at present a great desideratum in galvanism: the common pile, although at first sufficiently strong, very soon loses its power; it is besides very troublesome to be constantly repiling it, and clearing the different pieces from oxidation, &c. which must necessarily be done, if it is intended that the apparatus should produce the full effect. The contrivance which has succeeded best with me is as follows: I constructed a kind of trough of baked wood, 26 inches in length, 1.7 inches deep, and 1.5 inches wide; in the sides of this trough grooves were made opposite to each other, about the tenth of an inch in depth, and sufficiently wide to admit one of the plates of zinc and silver when folded together; three of these grooves were made in the space of one inch and three tenths, so that the whole machine contained 60 pairs of plates. A plate of zinc

\* See the former paper on this subject.



and silver, each 1,6 inches square, well cemented together, were introduced into each of these grooves or notches, and afterwards cemented into the trough by a composition of rosin and wax, so perfectly, that no water could pass from one cell to the other, nor between the plates of zinc and silver. This circumstance must be strictly attended to, else the machine will be extremely imperfect. When all the plates were thus secured in the trough, the interstices or cells formed by the different pairs of plates were filled with a solution of the muriate of ammonia, which here supplied the place of the moistened papers in the pile, but answered the purpose much better. It is hardly necessary to observe, that in fixing the zinc and silver plates, they must be placed regularly, as in the pile, *viz.* alternately zinc and silver, the silver plate being always on the same side. When a communication was made between the first and last cell, a strong shock was felt in the arms, but somewhat different from that given by the pile, being quicker, less tremulous, and bearing a greater resemblance to the common electrical shock. I constructed two of these machines, which contained in all 100 pairs of plates; these when joined together, gave a very strong shock, and the spark could be taken in the day time at pleasure; but what surprized me not a little, was the very slender power which they possessed in decomposing water; in this respect they were certainly inferior to a pile of 30 pairs, although such a pile would not give a shock of one third the strength.

This apparatus retained its power for many days, and would in all probability have retained it much longer, had not the fluid got between the dry surfaces of the metals. To remedy this defect, I have now soldered the zinc and silver plates together, and find that this method answers very well.

The zinc plates may be cleaned at any time, by filling the different cells for a few minutes with the dilute muriatic acid. Although this apparatus may not intirely supersede the pile, especially if it should be found to decompose water, &c. but slowly, yet in other respects it will no doubt be found very convenient and portable.

Having been lately favoured by Mr. Howard with narrow slips of platina, of a considerable length, I repeated the experiments respecting the decomposition of water, and with very nearly the same results. Seven measures of the compound gas obtained in the manner formerly described, being exploded over mercury by the electric spark, there remained no more than  $\frac{1}{3}$  of a measure, or  $\frac{1}{3}$  of the whole; this appeared to be azote, for it suffered no diminution with nitrous gas, nor was it inflammable. I likewise mixed one measure of this compound gas with two of nitrous gas, and the diminution amounted to one measure; nearly one third of the whole was therefore oxygenous gas, the remaining two thirds being hydrogen gas, with a little azote.

Having found that the solutions of metals in acids were decomposed by this influence, I next wished to ascertain if their solutions in alkalies, more particularly ammonia, could be decomposed in the same way. For this purpose, I added to a dilute solution of the nitrate of silver some pure ammonia, until the mixture smelled strongly of the latter substance. This mixture was introduced into a glass tube secured by corks in the usual way, and silver wires applied. When the tube was placed in the circle

of communication, a very rapid production of gas took place from the wire connected with the silver extremity of the machine, although but little or none escaped from the zinc wire during the whole process.

After some time a quantity of greyish flakes, evidently metallic silver, were separated by the wire which gave out the gas, and a dark grey powder was deposited on the zinc wire; at this period of the process much less gas was disengaged, although a considerable quantity still appeared at the silver wire. After some hours the apparatus was removed; at this time a very considerable quantity of metallic silver was deposited, and the zinc wire was encrusted with a blueish black substance. On endeavouring to remove this crust with the finger part of it exploded, although still moist: the wire was found to be very much corroded, and full of holes. Next morning the powder which adhered to the wire being dry, was touched with a knife, when it exploded again, with a very considerable noise. There can be little doubt, that these explosions proceeded from the fulminating silver of Bertholet, which must have been formed at the zinc wire; indeed some effect of this kind was expected. This experiment was repeated, and instead of a solution of silver nothing but pure ammonia was introduced into the tube, which answered equally well; for the silver of the zinc wire after being corroded, &c. was immediately taken up by the Vol. Alkali, and afterwards deposited in its metallic form by the silver wire. A small quantity of a fulminating substance likewise adhered to the zinc wire; and the fluid surrounding it, being poured into a wine glass, deposited after standing for some time; a black shining film, which when dried, detonated like Bertholet's powder, and resembled it in all respects.

Another tube containing pure ammonia had copper wires introduced into it; after this had been submitted to the action of the machine for some time, the fluid surrounding the zinc wire assumed a fine blue colour from dissolved copper, and the silver wire began to deposit metallic copper; this process continued for several hours, when a considerable quantity of very pure metal was precipitated; the blue colour of the fluid, however, never extended so high as the end of the silver wire, for as fast as it ascended, the metal was immediately thrown down in its metallic form, by this wire. I next filled the tube with an ammoniacal solution of copper, and employed copper wires. In a very short time after this had been exposed to the action of the machine, the upper part of the fluid in contact with the silver wire, became considerably paler, and copper was precipitated; in about an hour the whole of the fluid in this part of the tube became as colourless as distilled water, so completely had the metal been precipitated. The precipitated metal was also the purest I had ever seen. From these experiments it would appear, that the galvanic influence might be employed with success in the analysis of minerals, more particularly in separating lead, copper, and silver, from their different solutions; a very small quantity of a metal may likewise be detected in this way, as I have found from direct trial. Before I finish these observations on ammonia and ammoniacal solutions, I shall give the result of one experiment, where the vol. alkali itself was decomposed.



A quantity of pure ammonia being introduced into a bottle, and inverted in some of the same fluid in the manner already explained, the connecting wire on the zinc side was changed from silver to platina; in this case a very rapid production of gas took place from both wires, but the greatest from the silver wire. After two ounce measures of this gas had been collected, it was examined, and found to consist of,

Hydrogen gas	-	-	-	-	15 parts
Azotic gas	-	-	-	-	13
And oxygen gas	-	-	-	-	2 nearly.

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Indeed the quantity of oxygen was so small, that it could only be detected by nitrous gas, and it may have been originally contained in the fluid. In this case the ammonia must have been decomposed at the zinc wire, the disengaged oxygene uniting with the hydrogen, while the azote of the ammonia escaped in the form of gas, and mixed with the hydrogen disengaged at the same time from the silver wire.

In a former paper I mentioned that the sulphuric acid was decomposed by galvanism, without describing however the particular appearances. A quantity of the concentrated sulphuric acid was poured into a tube bent in the form of the letter V, two slips of platina were introduced, one into each leg, and brought within an inch of each other at the bend; the tube was then placed in an inclining position with the bend downwards, and the platina slips were attached to the extremities of the machine.

In a few seconds a very considerable quantity of gas made its appearance at the zinc wire, and continued to rise during the whole process; from the silver wire a small quantity of gas was likewise disengaged, but in a short time the fluid in this leg of the tube lost its transparency, and became milky and opaque. After the process had been continued for several hours, a quantity of a yellowish white powder was deposited on the silver wire, and the fluid still continued muddy; the acid, however, in the other leg, not only preserved its transparency, but became even more so; it likewise had a peculiar smell, somewhat like that of the ox. mur. acid when very dilute. The tube being removed, water was added to the muddy acid, in consequence of which a whitish powder was deposited, which was found to be sulphur. The yellowish powder deposited on the platina slip proved likewise to be sulphur. The gas obtained during this process was not particularly examined, it was only found not to be inflammable, or if it contained any inflammable gas, it must have been in very small quantity.

The effects of this influence upon the nitrous acid were not exactly what might have been expected; some of this fluid in its most concentrated and fuming state, being introduced into the bent tube just described, and the platina slips applied, very little or no gas was disengaged from the slips or wires of either side, nor even after some time, was the appearance of the acid sensibly changed. The influence, however, was perfectly transmitted, for the machine, although in full action, did not give the least shock whilst the tube

was applied; besides the wire on the zinc side being removed from the plate, and plunged in a cup containing water, whilst a copper wire reaching from the cup to the zinc plate, preserved the communication, gas issued copiously from the platina wire, and the copper was corroded as usual.

The acid was then diluted with an equal bulk of water, or rather more, and being again introduced into the tube, still very little gas was produced on either side. From these facts it would appear that the nitrous acid is so perfect a conductor of this influence as to transmit it like metals, without being at all acted upon by it. This may possibly be owing to the great proportion of oxygen which enters into its composition, for we have already remarked, that all fluids, containing very little or no oxygen, are perfect non-conductors, or nearly so; and it is probable that the conducting fluids are more or less so in proportion to their quantity of oxygen.

It was remarked in a former paper, that it was highly probable, the nitrous, or some other acid, was produced at the zinc wire, I shall now bring forward some additional facts and arguments in support of this hypothesis.

1st. If the tincture of litmus be introduced into the bent tube already mentioned, and platina wires employed; after some time the whole of the litmus in the leg of the tube connected with the zinc wire will be rendered perfectly red, and in some places the colour even partially destroyed, a well known effect of the nitrous acid on blue vegetable infusions. But if the litmus be mixed with a little of any pure alkali, no such change of colour can be perceived.

2dly. All metals which are dissolved or acted upon by the nitrous acid, are likewise very quickly corroded by the galvanic influence; but such as are not acted upon by this acid are not at all affected by this influence. Of this we have striking examples in silver, mercury, gold, and platina; the first two are readily acted upon both by the nitrous acid and galvanism, but the two last by neither in any sensible degree, and possibly not at all when perfectly pure. Now silver, which has always been considered as a perfect metal, cannot be oxydated by any of the usual methods, any more than gold, yet this influence has fully as great an effect upon it, as upon copper; which we conceive may be explained in the following manner:—Let us suppose that a little nitrous acid is produced on the surface of the silver, the metal must in this case be in some degree acted upon by it, and this action will necessarily dispose it to unite with oxygen, which, being thus in its nascent state, must readily combine with the silver, and form insoluble nitrate with excess of oxyde, which will, consequently, be but little soluble in water. Now that this is really the case, we think is rendered probable by the following experiments:

A small wide mouthed-bottle was filled with a dilute solution of pure pot-ash, and inverted in a cup, containing some of the same solution. Two silver wires, previously bent, were introduced a little way beyond its mouth, and then connected with the machine in the way formerly described. A very rapid production of gas took place from both wires, and a few black flocks were perceived floating in the fluid: when a sufficient quantity



quantity of gas was thus obtained, it was examined, and found to consist of oxygen eleven parts, and hydrogen twenty-five parts; and four measures of it being exploded by the electric spark, less than half a measure remained, or about one-ninth of the whole. From hence it is evident that the silver was but very little acted upon in this experiment, for we obtained nearly the same quantity of oxygen as if gold or platina wires had been employed. The bottle was next filled with distilled water, and the same process repeated. In this case the silver was much corroded, and very little gas escaped from the zinc side; a sufficient quantity of gas being, however, obtained, it was examined, and found to consist of oxygen one part, and hydrogen six parts. This mixture did not explode without the addition of more oxygen. The cause of these different results is readily explained on the supposition of an acid. For in the first experiment the generated acid would immediately unite with the alkali in preference to the silver, and by this means the principal agent in the oxydation would be removed. Similar experiments were made with copper wires, and with exactly the same results: in this case, when the bottle was filled with the alkali, no green substance was produced at the zinc wire; indeed, the copper was scarcely, if at all, acted upon.

3dly, The corroded substances generated by this influence at the extremities of the zinc wires are not pure oxydes, which, upon the supposition of simple oxydation they ought to be, when nothing but distilled water is employed. We have remarkable examples of this in the blueish green substance obtained from copper, and the dirty greenish yellow one from mercury, the last of which exactly resembles what has been called nitrous turbeth when too much washed with water, and if pure ammonia be added to it, it instantly becomes black, a proof that it contains some acid, for ammonia has no effect upon the pure oxyde of mercury, but a considerable one upon all the impure, rendering them black, as turpeth mineral, &c. With regard to the green substance from copper, it is very certain that we have no pure oxyde of this colour, for all the green oxydes, as they have been improperly called, contain acids of some kind, as the carbonic, arsenical, acetous, &c. and it is well known that the precipitates by the carbonated alkalis, &c. are all of this kind. Indeed, there is but one pure oxyde of copper, viz. the dark red, or deep brown. Now, as these impure oxydes cannot be generated without an acid, and as there can be none in distilled or lime water, it must follow, that an acid is somehow generated in the process; and as this acid acts both upon mercury and silver, it is most probably the nitrous. The quantity, however, is certainly small, owing, in a great measure, to the weakness and imperfection of our machines; and it is this circumstance which renders it so difficult to detect it with certainty.

It may be asked, how can this acid be formed? If azote were a compound substance, as has been lately affirmed, this question could be very readily answered; but if not, we must then suppose that a small quantity of azote is always present in water, however long boiled, or repeatedly distilled, and that it is with this the oxygen in its nascent state

unites and forms nitrous acid. The production of ammonia at the silver wire can be explained on either hypothesis; but we shall take another opportunity of examining this part of the subject.

The new apparatus which I described in a former paper, seems to answer better than was at first expected; it has now been in use occasionally for a month, and whenever a fresh solution of salt, or muriate of ammonia, is introduced, it appears to be fully as strong as at first. The addition of a very small proportion of nitrous or muriatic acid to the solution of common salt, adds considerably to its efficacy.

## V.

*Description of a Mercurial Air Pump; and of a Double-barreled Air Pump. By RICHARD AUGUSTUS CLARE, Surgeon, Jamaica. Communicated by the Author.*

**FIG. 1, Pl. XI.** represents a section of the different parts of the pump; A, a large iron tube (being the barrel of the pump) B a smaller ditto. These tubes are flanged to the iron step C. This step has a perforation in it, making a communication between the bottoms of the tubes. The tube B is surmounted by a plate of iron, on which is flanged the iron hemisphere D. The hemisphere has a hole at top, one-tenth of an inch in diameter, with a cap valve and basin at E. A pipe, *bb*, of iron or glass, is inserted through the upper part of the hemisphere, and reaches within a little of the iron plate on which it is flanged. The pipe *bb* rises about thirty-five inches above the level of the basin E; it is reverted at top, and descends to be inserted into the upper part of the piece G, which has a stop cock at *c*, leading to the receiver. To the lower part of the piece G, the long barometer gauge *d* is attached, the lower end of which descends into the basin of mercury *e*. F is a cylindrical piece of light wood (which I shall call the piston) made to slide easy in the hollow cylinder A. There is a small screw at *a*, to draw off the mercury when required. The valve at E is made of a thin iron hoop about three-fourths of an inch long, and half an inch in diameter; the lower end of this hoop is covered with a piece of bladder in the manner of a drum. The valve fits loosely into the cap, which has three grooves on the inside, made in a perpendicular direction from bottom to top. The use of these grooves is to allow the mercury to rise freely by the sides of the valve hoop. The use of the basin is only to contain the superfluous mercury. To try the power of this pump, let the piston be pushed down to the bottom of the cylinder, and retained there, as it will have a tendency to rise when the mercury is poured in. Take the valve out of the cap at E, and pour mercury into the basin till it rises as high as the top of the cap, then thrust down the valve, till the mercury runs over the top of its hoop and fills it. The specific gravity of the hoop so filled will be little less than that of mercury itself. Let the piston be now drawn



drawn up, the mercury will subside from the bottom of the valve, and make a vacuum in the hemisphere. No air can enter by the valve, as it will be always surrounded by and covered with mercury.

When the mercury sinks below the bottom of the tube *b b*, the air will enter the vacant hemisphere from the gauge or receiver. When the piston is depressed, the mercury will again rise in the hemisphere, and closing the orifice of the tube *b b*, will expel the air through the valve at *E*. The mercury will (as the exhaustion goes on) rise in the tubes *b b* and *d*, and thereby indicate the degree of rarefaction, but it can never rise so high as to run over the arch of the tube *b b*, because the column of mercury in that tube will be long enough to balance the weight of the atmosphere. As the oscillation of the mercury in the tube *b b* is apt to be very violent if not restrained, a small cork ball, with a short piece of iron wire struck into one side of it, may be placed under the orifice of that tube, the iron wire rising within the tube as represented at fig. 2. This ball will float on the mercury, and being thereby pressed tight against the orifice of the tubes, will prevent the mercury from rising too fast, or producing any sudden oscillation therein. The drawings and description of an air pump on this principle, reduced to a very portable size, were sent to Mr. Adams, mathematical instrument maker, of Charing-cross, London, in the beginning of the year 1796, who deemed the execution of it impracticable. Being informed of this, I desired the drawings, &c. might be laid before Mr. Nairne; who, though he offered to make the instrument, gave it as his candid opinion that it would not answer my purpose to the full extent of my wishes. I, therefore, dropt the idea of getting an instrument of this kind made in England, on account of the great distance and length of time necessary to carry on a correspondence on such a subject.

Mr. Nairne having given no reason, however, for his opinion, except that it would cost a great deal of money, I was resolved to attempt the fabrication of one myself. As I had no means of getting iron pipes made to the size I wished, I was obliged to make shift with what I could get. The tube *A A* was of cast iron (and had formerly belonged to a water pump) cut to the proper length. The tube *B B* was a gun barrel, and the rest of the instrument was made up in the same way, of any thing I could pick up. Being but a bungling workman, however, having but few tools, and no advice, or assistance, I never could make all the joints perfectly air tight, though I laboured hard to effect it.

I am fully convinced, however, from the experience I have gained with this clumsy and imperfect instrument, that it would answer the purpose if it were well constructed.

The greatest inconvenience attending the use of it was occasioned by the oscillation of the mercury, in consequence of the great difference in the diameters of the tubes through which it moves.

*Description of a Double-barreled Air Pump.*

Fig. 2. represents a section of the pump, &c. where A and B are the two barrels, open at top, and furnished with pistons, which are worked by rack and wheel in the usual manner. The pistons are alike in each barrel, and are perforated through the centre, as is the solid part of the rack also, to admit the hollow wire *a a* to slide therein. This wire is made to move air tight, by means of collars of leather in the centre of the piston. The wire is hollow from end to end, and is fixed to the centre of the bottom of the barrel. Near the upper end of the wire a hole is drilled through it, at right angles with that in its axis; this hole is at such a distance from the bottom of the barrel, that, when the piston is drawn up, the hole opens into the barrel, as is seen in the barrel B. Each piston is furnished with a common silk valve *g*, opening upwards. The hole in the piston rack, through which the wire slides, is secured at top by the stop screw *b*. The barrels are flanged on the bottom piece *c c*, into which the sliding wires are fixed as above described. The hollow wire in the barrel B communicates at bottom with the duct *d b*, leading through the cock *c*, and that in the barrel A communicates with the duct *e f*; *f* is the hole leading back to the receiver.

The cock *c* is perforated, as represented in the figure, and has a motion of one quarter of a revolution to the left, from its present position. The pistons should be well leathered, and always have about a quarter of an inch of oil on their upper surfaces. The operation of this pump is as follows:—Let the cock stand as represented in the figure, in which situation it opens a communication between the receiver and the barrel B, through the duct *c b* and sliding wire, when the piston is drawn up. When the piston B is depressed, the lateral hole in the sliding wire is stopped by passing through the collar of leather, and the air below it not being able to return into the receiver, is expelled through the valve *g* in that piston. By this time the piston A has arrived at the top of that barrel, and the air will have a free passage into it from the receiver, through the hollow wire, &c. On the depression of this piston the air below it will be expelled through its valve *g*, and thus by the alternate elevations and depressions of the pistons a pretty good exhaustion will be made: at length, however, it will cease to take more air from the receiver, in consequence of the small quantity of air in the valve holes and under the pistons that cannot be expelled. When this is the case, let the cock be turned one quarter round to the left, in which situation it will make a passage through the duct *d b*, at the same time that it cuts off the communication between the barrel B and the receiver, and the little air under the valve, &c. in the barrel A will immediately run into the exhausted barrel B through the duct *d b* and hollow wire *a a*. The pump A will now make a better vacuum, and take air again from the receiver, which will, when the piston A descends, be transferred to the barrel B; and, finally, when the piston B descends, the air will be expelled through the valve in its piston.



piston. Thus, by only turning the cock one way or the other, the cock is made to exhaust with one barrel or both. When it exhausts with one only, the other is rendered subsidiary to it by clearing it of air after each stroke.

To render the pump more easy in working, there should be a hole in the bottom of each barrel, furnished with a silk valve, through which the air would enter during the first strokes of the pump. The situation of these holes are shewn by dotted lines in the figure. The air may be restored to the receiver by unscrewing the stop screw *b*.

Being foiled in all my attempts to procure a mercurial air pump, I turned my thoughts to the improvement of the common double barreled pump, which, I believe, was invented by Dr. Robert Hooke; and which appeared to me the most simple and convenient form, provided it could be made more accurate in its operation.

The result of some thought, and various experiments made to this purpose, was, that in theory I brought that pump to the perfection of rarefying permanent dry air 248000000 times, supposing the air to have a uniform power of expansion to such a degree, and the barrels of the pump to be twelve inches long by two in diameter. The drawings and description of this pump are now in the hands of Mr. Nairne, who is making one on that construction, particularly adapted to the performance of chemical experiments on air. I have been purposely short in my description of the scheme herewith sent (which differs in many respects from that making by Mr. Nairne) as I purpose giving a full description of the other and its performance (if I find it to equal those hitherto contrived) when the instrument comes to my hands.

Query, does the lower valve of Mr. Cuthberton's air pump increase the power of that instrument, and would it not work equally well without it?

My reasons for suspecting the utility of this valve are the following:

First, there must always be a quantity of air above the piston, when drawn up, that cannot be expelled.

Secondly, this quantity of air will be uniformly the same, whether at the beginning or end of the exhaustion, as it will take the same force to open the expulsion valve at one time as at another, and this air must be as dense as that of the atmosphere at least.

Thirdly, when the piston begins to descend, the large conical valve must thrust out of its shell, before that at the bottom of the barrel can possibly shut, for the shutting of the one depends on the motion which causes the opening of the other.

Fourthly, when the piston begins to ascend, the valve at the bottom of the barrel must open before that in the piston can shut, for the reason above stated.

Now let us suppose the piston at the top of the barrel, when the winch is turned the large conical valve will be opened, before that in the bottom of the barrel can shut, at least there will be an instant when both valves may be said to be half open, the air above the piston being much denser than that below it, will take advantage of this opportunity to diffuse itself equally through the barrel and receiver. If, however, the air does not all

escape at the commencement of the down stroke, it will have another opportunity at the commencement of the up stroke, for then the bottom valve opens before that in the piston can shut, and the air in the barrel will again be at liberty to exert its superior spring to get back into the receiver.

It is with the greatest diffidence I put this question, having the highest respect for Mr. Cuthberton's abilities.

If I have advanced what is erroneous, or if I have mistaken the principle of the instrument, I beg to be corrected and informed.

## VI.

*A Memoir, in which the Question is examined, whether Azote be a simple or compound Body? By CHRISTOPHER GIRTANNER, Doctor of Physic at Gottingen.*

(Concluded from page 171.)

FROM this experiment we see that pure hydrogen gas combines with the oxygen gas which had remained in the lungs, and forms azote gas.

16. Another experiment, which appears to me to confirm my opinion, was made by Mr. Henry of Manchester. He repeated it several times, and always with the same success. His explanation of this phenomenon may be seen in Scherer's Journal, which is different from mine, because he considered azote as a simple body. His experiment is as follows:—In a recurved tube over mercury a mixture was made of 94,5 measures of carbonated hydrogen gas from acetite of pot-ash, and 107,5 measures of very pure oxygen gas obtained from the oxygenated muriate of pot-ash. The mixture amounting to 202 measures, was reduced, by an electric explosion passed through it, to 128,5 measures, and afterwards by lime water to 54,0 measures. So that in this experiment 23 measures of azote gas were produced by the electric explosion of the oxygen united with the hydrogen.

17. Of all known bodies zinc is, if I am not deceived, that which most easily unites with oxygen; it takes it from almost every other body, and this property renders it highly useful for the exhibiting the minutest quantities of oxygen. It was more particularly by means of zinc that I succeeded in separating the oxygen of the muriatic acid from its base. I have likewise used it to make the last analysis of ammoniac. Fillings of zinc are to be mixed in a retort, with concentrated fluid ammoniac. A communication being thus made between the retort and the pneumatic apparatus, the whole being kept in digestion for several days, taking care not to raise the heat too much, the ammoniac is decomposed. The oxyde of zinc remains in the retort, and in the pneumatic apparatus a considerable quantity of hydrogen gas is obtained, together with a small quantity of ammoniacal gas and of undecomposed azote. It is easy to prove that the hydrogen gas was

not



not obtained from the water in which the ammoniac was dissolved; because the quantity of the gas is too large, and that of the azote too small, to admit of any doubt respecting the decomposition of the latter.

Such are the experiments, from which it appears to me, that we are justified in concluding, that azote is a compound of oxygen and hydrogen. I must refer to a second memoir for several other experiments no less decisive, which require to be repeated before I submit them to the examination of those accurate and enlightened chemists, to whom this memoir is addressed.

Azote being, therefore, if I am not deceived, a body composed of hydrogen and oxygen, it follows:

That the atmosphere is not, as has been hitherto supposed, a mixture of oxygen and azote gases, but rather a mixture of oxygen and hydrogen gases; water in form of gas, if I may be permitted to use this expression.

When, by those chemical experiments which have been improperly called eudiometrical, the oxygen is separated from the hydrogen, this separation can never be totally or completely made. Part of the oxygen remains united to the hydrogen, and forms that chemical combination which we call azote, and obtain in these experiments. The oxygen, so indispensable for supporting the life of organized beings, is changed by its combination into hydrogen, into azote, which is not only incapable of supporting life, but is a true poison, in consequence of the affinity it has with oxygen, and the avidity with which it takes it from organized bodies.

Oxygen has so strong an affinity with hydrogen when both are mixed in the atmosphere, that it is very difficult to separate them entirely, and hence the great difficulty of analysing azote. When charcoal, sulphur, a lighted candle, or the metals, have ceased to burn in atmospheric air, when animals have perished in a given quantity of this fluid, the remainder still contains a notable proportion of oxygen. Phosphorus burns in this residue very well, and for a considerable time: and even when the phosphorus has ceased to burn, there always remains a small quantity of oxygen united to hydrogen, that is to say, there is a remainder of azote. We may, nevertheless, as I have several times observed, deprive the atmospheric air of almost the whole of its oxygen, and render the analysis nearly complete by heating phosphorus in it for a certain time. Phosphorated hydrogen gas is then obtained by the change of part of the azote into hydrogen.

From the experiments here recited, it follows, that the eudiometer, such as it exists at present, is founded on erroneous principles, the azote obtained in these experiments being always a product of the operation, which did not exist before in the form of azote previous to the examination. Von Humboldt, who is much disposed to draw general conclusions from insulated facts, appears to have been deceived when he advances that the earths may be used to determine<sup>the</sup> the quantity of azote contained in atmospheric air. The earths do not point out the azotic part; but change a portion into azote.

Cultivated earth, brought into contact with the atmosphere, absorbs the oxygen, and forms azote. In countries situated near the poles, and on the top of the Alps, where the earth is always covered with snow, the atmospheric air contains a greater quantity of oxygen than in southern or low countries, because the snow prevents the air from coming into contact with the earth.

The constituent principles of azote being now known, we are enabled to form a new theory of the art of making nitre. In the *Annals of Chemistry*, Vol. XX. I observe some valuable observations on this subject. For instance, p. 313, "Earth extracted from deep subterraneous places, inaccessible to the light, such as caverns, requires only to be exposed to the air a few days to produce salt-petre in abundance. It is proper to observe, that these earths do not furnish a single atom of nitre when they are first taken from the moist and dark place where they were formed; and that it is only by combination or combustion of the azote of the earth with the oxygen of the atmosphere that this salt is produced."

The explanation which is given, page 314, of that interesting phenomenon, is a proof how distant we were from the truth before the nature of azote was understood.

As lime absorbs oxygen with avidity, we see the cause why rooms that have been newly white-washed are unwholesome.

The following bodies are combinations of hydrogen with oxygen in different proportions:

Azote.	Oxygenated muriatic acid.
Gaseous oxyde of azote.	Nitro-muriatic acid.
Nitrous gas.	Water.
Nitrous acid.	Atmospheric air.
Nitric acid.	Ammoniac.
Muriatic acid.	

I hope soon to prove that pot-ash, soda, and sulphur, ought to be added to that list. Phosphorus appears to me to be hydrogen, in the purest state in which we are acquainted with it; but this I am very far from having in my power to prove. The analysis of fixed alkalis is completed. As, however, I have no intention of saying any thing upon that subject in the present article, I shall confine myself to pointing out one of the decisive experiments. When an alkali is fused with siliceous earth to make glass, the alkali is analysed. The hydrogen escapes under the form of gas, and the oxygen becomes combined with the siliceous earth; glass being merely an oxyde of silic. Too great a quantity of oxygen should not be admitted, as it renders it less transparent; for this reason it is usual to add substances which strongly attract oxygen, such as manganese.

Mr. Mayer, to whom I had communicated the result of my experiments, by which I so evidently confirmed an idea which he had ventured to throw out, and of which he has the merit of being the author, sent me a paper, from which I shall give an extract:

"I am inclined," says he, "to think with Mr. De Luc, that the evaporation of water, as it continually takes place on a large scale in nature, is a real conversion of the water  
"into



“into air. It is true, that we have not been able in our laboratories \* to accomplish the  
 “changing of water into evaporation. This arises from our being ignorant of the share  
 “which electricity and light have in this natural process. Nevertheless, it appears pro-  
 “bable to me, that the ponderable parts of atmospheric air, that is to say, oxygen and  
 “azote has no other source than the water with which the surface of the globe is covered.  
 “The very small quantity of oxygenous gas which is exhaled by vegetables by means of  
 “the sun, is far from being able to repair the enormous consumption of that principle  
 “which daily takes place in our atmosphere in so many different ways. Now, supposing,  
 “according to this theory, that 100 grains of water are changed, by the unknown process  
 “of nature, into 100 grains of atmospheric air, that is to say, into a chemical mixture of  
 “oxygen and hydrogen, we can find, by a very simple calculation, in what proportion  
 “oxygen and hydrogen are combined to form the azote which we observe in the atmo-  
 “sphere. Let us not, however, forget that the nature of what we call azote is not always  
 “the same, and that it varies considerably in the proportions of its constituent prin-  
 “ciples †.

“I shall call water E, oxygen O, hydrogen H, the ponderable part of the atmospheric  
 “air L, the ponderable part of azote gas S.

“Then  $100 E = 85 O + 15 H$ .

“But a cubic inch of atmospherical air is composed of  $\frac{1}{4}$  of a cubic inch of gas,  
 “and  $\frac{3}{4}$  of a cubic inch of azote gas; therefore

$$1 L = \frac{1}{4} O + \frac{3}{4} S.$$

“One inch of atmospherical air weighs 0.46 grains.

“One inch of oxygenous gas weighs 0.51 grains.

“One cubic inch of azote gas weighs 0.44 grains; consequently

$$0.46 L = 0.51 \frac{1}{4} O + \frac{3}{4} 0.44 S.$$

$$1.84 L = 0.51 O + 1.32 S.$$

$$184 L = 51 O + 132 S.$$

$$100 L = 27.8 O + 72.2 S.$$

“Azote gas being a compound of oxygen and hydrogen.

$$S = x O + y H; \text{ so that}$$

$$100 L = (27.8 + 72.2 x) O + 2 y H.$$

“Now nature changes 100 grains of water into an hundred grains of atmospherical  
 “air; consequently

$$100 E = 100 L; \text{ whence}$$

$$27.8 + 72.2 x = 85, \text{ and}$$

$$72.27 = 15;$$

\* See, however, Priestley, in *Philos. Journal*, IV. 193, and elsewhere.---N.

† This is a very important point upon which Mr. Mayer fixes our attention. It will be necessary to make a great number of very exact experiments to determine the different degrees of the oxydation of hydrogen, and to distinguish the different gases comprehended, till now, under the general name of azote gas.

Then.

Then  $x = 0.79$ , and  $y = 0.21$  nearly.

$S = 0.79 \text{ O} + 0.21 \text{ H}$ , or  $100 \text{ S} = 79 \text{ O} + 21 \text{ H}$ .

"That is to say, 100 grains of azote gas are composed of 79 grains of oxygen and 21 grains of hydrogen."

Having applied this ingenious calculation of Mr. Mayer to other oxydes of hydrogen, I found as follows:

Let N denote the nitric acid: we know by experiments that

$100 \text{ N} = 79.5 \text{ O} + 20.5 \text{ S}$ .

Instead of S, let us put its value, as found by the preceding calculation, and we shall have

$100 \text{ N} = 79.5 \text{ O} + 20.5 (0.79 \text{ O} + 0.21 \text{ H}) = 95.7 \text{ O} + 4.3 \text{ H}$  nearly.

Thus 100 grains of nitric acid contain nearly 96 grains of oxygen, and 4 grains of hydrogen.

The following are the results of the same calculation:

100 parts of ammoniac = 63.72 oxyg. + 36.28 hydrog.

100 parts of azote = 79.00 oxyg. + 21.00 hydrog.

100 parts of atmosph. air 84.67, oxyg. + 15.33 hydrog.

100 parts of water = 85.66 oxyg. + 14.34 hydrog.

100 parts gaseous oxyde of azote = 86.77 oxyg. + 13.23 hydrog.

100 parts of nitrous gas = 93.28 oxyg. + 6.72 hydrog.

100 parts of nitrous acid = 94.33 oxyg. + 5.67 hydrog.

100 parts of nitric acid = 95.70 oxyg. + 4.30 hydrog.

Are then oxygen and hydrogen the two elements of which all the bodies in nature are formed. It appears to me very probable. It appears also probable that the heavier a body is the more oxygen concentrated and deprived of caloric it contains, and that, on the contrary, the more hydrogen a body contains, the lighter it must be. But these are only hypotheses which future experiments will either confirm or replete.

It seldom happens that the two constituent principles of azote enter, as simple bodies, into other combinations; they much oftener form one single ingredient by their union in different proportions. Coal, or the oxyde of diamond, is found in many bodies, and the diamond itself in none with which we are acquainted. We obtain coal in our chemical decompositions, and never the diamond. We do not even know such a thing as diamantic or adamantic acid, although carbonic acid is familiar to us. No chemist would think of affirming that we exhale diamond in the act of respiration, but that we exhale a quantity of coal. The diamond itself is not, perhaps, a simple body; it probably contains oxygen; since all transparent bodies, if I mistake not, contain more or less of it. Azote, like coal, is susceptible of various combinations merely as azote; as a compounded principle, which gives to bodies properties very different from what is given to them by the combination of the two simple principles of which azote is composed. Azote, as a constituent principle of bodies, is not hydrogen and oxygen, but azote: exactly as soap is not oil and alkali, but soap. We must never lose sight of this.

I have



I have observed, in several experiments, that sulphur frequently assumes a yellow, yellowish, or orange colour. We are seldom deceived in supposing that there is sulphur in bodies which exhibit that colour. Those bodies which contain coal are of a black, brown, or a violet colour. Azote discovers itself by a green colour. Plants (*etioleés*) which have vegetated, defended from the air and light, are white, and contain very little azote. Expose them to these two agents for a few days, they assume a green colour, and from that time the proportion of azote in their composition becomes much more considerable. When nitrate of pot-ash is fused in a glass retort, and the oxygen gas is separated, it is observable at the end of the operation, whilst the azote is forming, that is to say, when the oxygen has a greater affinity to hydrogen than to caloric, that the green colour appears, and the separation of the oxygen gas entirely ceases.

But what demands the attention of every philosopher is, that when water is exposed to the sun, the light decomposes it, and separates the oxygen in large quantities. The hydrogen retains the last particles of the oxygen; azote is formed, and announces itself by a green colour. More and more of the water is decomposed; the oxygen (which I have already shewn to be the principle of life and irritability in organized nature) becomes more fixed; and this azote, produced from the effect which the sun has upon water, becomes an organized body, the *conserva fontinalis*, a plant which lives, grows, and perpetuates its species. Here are the limits at which philosophy must stop—to admire and respect the secrets of nature, without knowing if we shall ever be permitted to penetrate them. It is certain, and I have satisfied myself respecting it by a great number of experiments in which I have been engaged every summer for the space of more than twelve years, that the influence of the solar light is absolutely necessary for changing the water into a plant, or organized azote. No degree of heat can supply the place of it, and this experiment alone ought to be sufficient to convince every unprejudiced reasoner that heat and light are two substances totally different. I am surprized that this green matter of Priestley, this *fontinalis*, to which I have given the name of organized azote, has not engaged more of the attention of chemists. It is the most wonderful substance which exists, the most singular body in nature. Nothing can be more absurd than what Priestley has said concerning it. To reason as he has done, is, indeed, not to reason at all. This great man, whose name will live as long as the sciences shall be cultivated, has made the most eminent discoveries. I admire his talents, but I am sorry to observe, that in all his productions he appears to be more of an experimentalist than a philosopher. Whilst with one hand he unfolds to us the astonishing secrets of nature; with the other he is always ready to shut our eyes in case we have a desire to penetrate further than he wishes us to see. In the dispute which he maintained with Dr. Ingenhousz, concerning the subject of the green matter about which we are speaking, he has given a striking instance of his disinclination to permit the wonders of nature to be beheld, except through his ecclesiastical perspective. Ingenhousz, an enlightened chemist, having described several experiments, worthy the attention of every thinking being, relating to this singular substance, adds,

"Water itself, or some substance in water, is, in my opinion, changed into this species of vegetation. It is a real transmutation, which may appear incomprehensible to a philosopher, but which, in reality, is not more extraordinary than the transformation of grass and other vegetables into fat in those animals which feed upon plants, or than that of the aqueous juice of the olive into oil."

Water is changed into a plant. This is the fact. Ingenhoufz stops there, and says, I do not comprehend it; this is the language of a philosopher. Priestley, on the contrary, is scandalized at such language. He asks Ingenhoufz if he is not ashamed to wish to re-establish the long refuted doctrine of spontaneous generation\*.

He speaks to him nearly in the language of the inquisitor to Galileo, when he proved to that immortal man that the sun turns round the earth. Priestley afterwards gives us his own theory of the production of the organized azote. He says, that the seeds of this plant float every where in the air, over the earth, the sea, the Alps, the low ground, at the poles, and at the equator, in summer, in winter, and in every season, and that they are received into water where they germinate. But organized azote is produced in bottles well stopped. Dr. Ingenhoufz has even proved that if a bottle be filled with well water, and reversed over a basin filled with water, a great quantity of organized azote will be formed. Priestley, who could not, without attributing a superior intelligence to the pretended seeds of this plant, maintain that they came through the water purposely to fix

\* Dr. Priestley says, that the theory of spontaneous generation is a doctrine long since refuted. These are his words:---"Considering how long the doctrine of equivocal, or spontaneous generation has been exploded." A philosopher should never make use of such an expression. There is not an exploded opinion which we may not recur to and re-examine. The philosopher acknowledges no authority which prohibits him from admitting any opinion, or forbids him to examine it. There are many other opinions long since exploded to which we must now have recourse. That, for instance, of the transmutation of metals. What chemist, of the present day, would presume to deny the possibility of it: The change of one metal into another should appear less difficult than the changing of the sweetest body (sugar) into the fourest body (the oxalic acid); than the change of the hardest body (the diamond) into the softest body, the carbonic acid gas; or than the changing the most transparent body, the diamond, into coal, the most opaque of all bodies. In the nineteenth century, the transmutation of metals will be generally known and practised. Every chemist, every artist, will make gold. The utensils for our kitchens will be made of silver, or even of gold, and this will contribute, more than any thing else, to prolong life, which has hitherto been poisoned by the oxyde of copper, lead, and iron, which we daily swallow at our meals. There will then be no other wealth than natural wealth, the production of the soil. Artificial wealth, such as gold, silver, and paper, will be extinguished in the hands of those who shall have accumulated them. What a revolution in society! But every enlightened chemist will agree with me, that this revolution is not only probable, but near.---Note of the author.

Though I have no remarks to offer on this last assertion of our author, I cannot forbear remarking that gold and silver, though much used as signs of wealth, or tickets of commercial transfer, have always, and more especially in modern times, constituted a very small portion of national wealth. If gold could be rendered as cheap as iron, the effect upon the wealth of society would be incomparably less than has resulted from either of the inventions of the stocking loom, the spinning gear, or the steam engine.---N.

themselves.



themselves into this bottle which was prepared for them, extricates himself by making them pass through by imperceptible cracks\*. Such are the means which are always used when we will not see that which is too obvious. Thus it was that phlogiston was made to pass through crucibles, and azote through tubes and retorts.

I must observe, that in order to succeed in these experiments, it is absolutely necessary that the water which is used should contain gas in a state of dissolution, whether it be oxygenous gas, or carbonic acid gas. The more gas it contains, the greater quantity of organized azote will be formed.

It is to be wished that the change, which earths undergo from the oxygen which they absorb in decomposing the vapours of water, were attentively investigated. I do not doubt but that such an investigation would lead to some very important discoveries.

I submit these ideas to the enlightened criticisms of those illustrious French chemists, the editors of the *Annales de Chimie*, the fathers and creators of this science. Before their time, chemistry was only a crude mass of facts, ill arranged, and worse explained.

## VII.

*An Account of some Experiments made with the Galvanic Apparatus of Signor Volta. By Mr. DAVY, Superintendent of the Pneumatic Institution. Communicated by the Author.*

IN pursuing a course of experiments on the galvanic influence I have made some new observations. They are connected with the curious facts already detailed in Mr. Nicholson's *Philosophical Journal*, and they may possibly lead to elucidations of the phenomena.

The apparatus that I employed was constructed for Dr. Beddoes, and never consisted of less than 110 pairs of metallic plates. I found the sensible galvanic shock very much increased when the parts communicating with the conductors were moistened with solution of green sulphate of iron. A pile erected with pieces of cloth, wetted in that substance, acted rather more intensely than a similar pile erected in the usual mode. It, however, lost its powers in a shorter time: perhaps a solution of sulphate of zinc may be employed with advantage.

a. Struck with the curious phenomena noticed by Messrs. Nicholson and Carlisle †, namely, the apparent separate production of oxygen and hydrogen from different wires, or from different parts of the water completing the galvanic circle, my first researches were directed towards ascertaining if oxygen and hydrogen could be separately produced from quantities of water not immediately in contact with each other:

\* "Through some unperceived fracture," Vol. I. p. 294. "The seeds of this plant insinuate themselves into vessels of water, through the smallest apertures," p. 308.

† *Philos. Journal*, page 183, Vol. IV.

Two silver wires, one from the zinc end of the apparatus, and the other from the silver end, were made to communicate with two glasses, distant from each other about five inches, and filled with water that had been long boiled, and was yet warm. Into one of these glasses I dipped the fingers of my right hand, and into the other the fingers of my left, so that the communication between them was made through my body. Immediately after the shock, the zinc wire\* began to calcine very fast, white clouds diffusing themselves from it through the water. At the same time gas was formed round, and extricated from the silver wire in the other glass. The communication was kept up for half an hour; during this time no gas was produced from the zinc wire, which continued to calcine throughout the process. The gas from the silver wire was caught in a small inverted cylinder; examined by the test of nitrous gas, it appeared to contain no oxygen, and inflamed with twice its bulk of common air, gave such a diminution, as denoted it to be hydrogen nearly pure.

This apparatus being adjusted as before, the communication between the glasses was made through three persons; the process went on, though less rapidly; the oxygen was fixed as before by the silver in one vessel, whilst the hydrogen was given out in the other. When muscular fibre, living vegetable fibre, or a moistened thread not exceeding three feet in length, was employed as the medium of connection between the glasses, similar effects were produced, though the gas was evolved more slowly than when the living animal was the connector. Muscular fibre appeared to be a better conductor than vegetable fibre, and vegetable fibre a better conductor than the moistened thread.

b. Several glass tubes about  $\frac{1}{2}$  of an inch in diameter, and four inches long, having each a piece of gold wire inserted into one end hermetically sealed, and the other end open, were provided. Two of these tubes were filled with distilled water, and inserted into separate glasses filled with that fluid. The glasses were made to communicate by means of fresh muscular fibre; the gold wires were connected with the machine by means of silver wires; one with the silver end, the other with the zinc end. Gas was immediately given out from both the gold wires; but most rapidly from that connected with the silver. In four hours and a quarter the process finished; the water in the tube communicating with the silver being below the gold wire. The gases were examined; the quantity from the water communicating with the zinc was equal to 33 grain measures, that from the water connected with the silver to nearly 65 grain measures. The zinc gas was mingled with 80 measures of nitrous gas, containing  $\frac{1}{22}$  nitrogen; a rapid diminution took place, and when the residuum was exposed to solution of green muriate of iron †, not quite five mea-

\* To prevent unnecessary repetitions, after Mr. Cruickshank, I have called the wire connected with the zinc end of the apparatus, the zinc wire, and that connected with the silver end, the silver wire.

† Solution of green muriate of iron rapidly absorbs nitrous gas, without effecting any change in it at common temperatures, and it is possessed of no action on gases not absorbable by water, and incapable of supporting flame. Solution of green sulphate of iron likewise absorbs nitrous gas without decomposing it. See *Research. Chem. & Phil. concerning Nitrous Oxide*, page 179. *Johnson*.



tures remained. Hence the 33 measures of gas evidently contained more than 31 measures of oxygen. The 61 measures in the other tube, gave with nitrous gas a diminution but barely perceptible, and fired after the absorption of the nitrous gas by the electric spark, with 60 measures of oxygen, left a residuum nearly equal to 36 measures; hence the gas was hydrogen almost pure.

c. There was every reason to suppose, that the slight diminution produced by the mixture of the hydrogen with nitrous gas in the last experiment, as well as the residual gas of the oxygen, were owing to common air held in solution by the distilled water, and given out from it during the process. To ascertain if the gases could be obtained perfectly pure, when water deprived of its loosely combined air by boiling was employed. The two tubes were filled with water that had been boiled for more than eight hours, and that was yet so hot as to be painful to the fingers; the glasses were filled with water of the same kind, and the process conducted as before; the tubes being suffered to cool before the communication was made, gas was given out very rapidly from the water connected with the silver; but very slowly from that connected with the zinc. During the whole of the process, no globules of air formed on the sides of the tubes, as in the last experiment. In five hours the tube connected with the silver contained 56 grain measures of gas. That connected with the zinc contained only 14 measures. The 56 measures gave no diminution with nitrous gas, and appeared by the test of detonation, to be pure hydrogen. The 14 measures tried by the tests mentioned in the last experiment, appeared to be oxygen, mingled with no perceptible quantity of other gas. In this experiment, as in the last, the gold wires were not apparently acted upon, nor was their color in the slightest degree altered; the deficient proportion of oxygen, there was every reason to believe, was owing to the absorption of that gas in the nascent state by the boiled water. Boiled water was now exposed to, and agitated in oxygen over mercury, till it was judged to be saturated with that gas. The tube connected with the zinc was filled with this water; the other tube was filled with common boiled water. The galvanic process was continued seven hours. In this time the water connected with the zinc had given out 27 grain measures of oxygen, apparently pure; from the water connected with the silver, 57 measures of hydrogen had been extricated.

d. Having thus ascertained that oxygen and hydrogen, nearly in the proportions required to form water, could be separately produced from quantities of water, having no communication with each other, except by means of the dry metallic conductors and muscular fibre. I next endeavoured to ascertain, if the contact of the metallic wires, with the metallic plates of the apparatus, were essential to the effect. The conducting ends, *i. e.* the silver and zinc, were made to communicate with two glasses of water, by means of two unconnected pieces of muscular fibre. A piece of silver wire was made the medium of connection between the glasses. Immediately after the connection, I was surprized by seeing that end of the wire in the water communicating with the silver calcining; whilst gas was given out from that part of it in the vessel communicating with the zinc; as was  
the

the case in the broken circuit described by Mr. Nicholson. When the tubes with gold wires, connected by silver wires, were employed, oxygen was given out in the water connected with the silver, and hydrogen in that communicating with the zinc. In none of these experiments could any production of gas from the muscular fibre be perceived; but the parts exposed to the water became whiter than before.

When (the glasses being connected with the apparatus by muscular fibre) the communication between them was made through my body by means of the gold wires in the tubes; one being in contact externally with my right hand, the other with my left, oxygen was produced as before in the glass connected with the silver, and hydrogen in that connected with the zinc. When I made the communication, holding a silver wire partly plunged into the water connected with the silver, in my right hand, the fingers of my left hand being in the other glass, the silver wire became slowly oxydated, and no gas was perceptibly given out in either of the glasses. When on the contrary, I introduced my hand into the silver glass, and the wire into the zinc glass, gas was given out round the wire, no oxydation took place, and no gas was extricated in the silver glass.

When the glasses were made to communicate both with the machine, and with each other, by means of muscular or vegetable fibre; and metallic wires introduced into either or both of the glasses, and wholly or partially covered with water, no gas was given out from them, and no apparent chemical change took place.

Reasoning on this separate production of oxygen and hydrogen, from different quantities of water, and on the experiments of Mr. Henry, junr. on the action of galvanic electricity on different compound bodies\*, I was led to suppose, that the constituent parts of such bodies (supposing them immediately decomposable by the galvanic influence) might be separately extricated from the wires, and in consequence obtained distinct from each other.

*a.* I filled two of the small tubes mentioned in *b. i.* with strong solution of caustic pot-ash, and inverted each of them in a glass filled with the same substance; the glasses were made to communicate with each other by means of muscular fibre, and the gold wires in the tubes connected with the ends of the pile. Gas was produced much more rapidly in this process from both wires, than in the experiment with simple water. In three hours no deposition had taken place in either of the glasses, nor were the gold wires sensibly acted upon. The gas given out in the tube connected with the zinc, measured exactly 37 grain measures, and proved to be oxygen absolutely pure, for with 80 measures of nitrous gas, containing about  $\frac{1}{3}$  nitrogen, it diminished to less than 3 measures. The gas given out in the tube connected with the silver, was equal to rather more than 72 grain measures. It gave no diminution with nitrous gas, and two 20 grain measures of it, fired with rather more than one 20 grain measure of oxygen, containing about .06 nitrogen, left a globule of air hardly perceptible.

\* Phil. Journ. Vol. IV. p. 224.



Surprized at these results, which proved that no decomposition of pot-ash had taken place, and that that substance in this mode of operating, only enabled the galvanic influence to extricate oxygen and hydrogen more rapidly from water, I was induced to operate upon this substance in the way of direct communication. Two gold wires were passed through holes in the side of a small glass tube closed at one end, and cemented so as to be distant about the eighth of an inch. This tube was filled with solution of pot-ash, and inverted in glass filled with the same substance; the gold wires were made to communicate with the ends of the pile. Gas was produced rapidly from both wires, but most from the silver wire; the gold was not acted upon, and no deposition took place. When near a quarter of a cubic inch of gas had been collected, it was transferred to a detonating tube, and fired by the electric shock over mercury, it gave a vivid inflammation, and left a globule of air not equal to  $\frac{1}{80}$  of the whole quantity.

b. Solutions of caustic ammoniac were exposed to the galvanic influence in the two tubes with gold wire, and connected by muscular fibre. Gas was given out very slowly in the tube connected with the zinc, and the gold wire was evidently acted upon, being in some parts corroded, and in other parts covered by a yellow deposit. In the tube connected with the silver, gas was given out more rapidly, and the gold was not altered in appearance. In five hours the gas in the zinc tube was equal to 5 measures, and proved to be a mixture of nearly 3 oxygen, and 2 of nitrogen. The gas in the silver tube measured 31 grains, and appeared to be hydrogen, mingled with a minute quantity of nitrogen. I repeated this experiment several times, to ascertain if after the solution of ammonia had been long galvanized, the proportion between the gases would be different. The gas given out in the zinc tube was always to that in the silver tube nearly as 1 to 6, but the quantity of oxygen appeared to increase towards the end of the process. A quantity of solution of ammoniac, the same as that used in the experiment exposed to heat in a mode described in *Research. Chem. & Phil.* page 241, readily produced  $\frac{1}{20}$  of its bulk of unabsorbable gas, which gave no diminution with nitrous gas, and appeared to be nitrogen. In every experiment yellow deposit was formed upon the gold in the zinc tube; muriatic acid poured upon some of this yellow deposit slowly dissolved it, a little nitrogen being given out during the solution.

When a solution of caustic ammoniac was exposed in the silver tube, and water in the zinc tube, the gold wires were not perceptibly altered, and oxygen was given out in the zinc tube and hydrogen in the silver tube, nearly in the proportions required to produce water. When on the contrary water was connected with the silver, and solution of ammoniac with the zinc, hydrogen was produced from the water; the zinc gold wire was corroded, and the mixture of oxygen and nitrogen to the hydrogen, as six to one, (one to six?) was produced as before.

c. Concentrated sulphuric acid was galvanized in the double tubes connected by muscular fibre. The gold wire in the zinc tube was not dissolved or corroded, and in a great length of time 41 measures of gas, which proved to be pure oxygen, were given out from

it. But little gas was given out in the silver tube, the acid in it was clouded with a white substance, which was evidently sulphur; it was perpetually produced round the point of the wire. 15 measures of gas were evolved, which gave rather greater diminution, fired with oxygen, than pure hydrogen; hence they might possibly have been partially sulphurated hydrogen.

When solution of caustic pot-ash was put into the silver tube, and sulphuric acid into the zinc tube, pure hydrogen was disengaged in the silver tube, and pure oxygen in the zinc tube; the same phenomena took place when water was employed instead of solution of pot-ash.

When water was connected with the zinc, and sulphuric acid with the silver, the products were the same as when pure sulphuric acid was used in both tubes. When very diluted sulphuric acid was employed in both tubes, oxygen and hydrogen, nearly in the proportions required to form water, were separately evolved.

d. The tubes were filled with pure solution of muriatic acid, and the communication made as before. No gas was given out in the zinc tube, and the gold in it was very much corroded; 30 measures of pure hydrogen were collected in the silver tube, the gold in which was not perceptibly acted upon. When water was made to communicate with the zinc, and muriatic acid with the silver, neither of the gold wires were acted upon, and 22 measures of oxygen were collected from the water, and 41 of hydrogen from the muriatic acid. When water was connected with the silver, and muriatic acid with the zinc, the same phenomena took place, as when pure muriatic acid was used in both tubes.

e. Concentrated solutions of nitric acid were galvanised in the tubes, 19 measures of pure oxygen were produced in the zinc tube. A globule of gas only, not equal to half a measure, was produced in the silver tube, and the acid became green on the top. The globule of gas did not diminish with oxygen, it was too small to be tried by other tests. The gold wires were not acted upon.

When nitric acid was connected with the zinc, and water with the silver, oxygen and hydrogen were separately produced. When water was connected with the zinc, and nitric acid with the silver, oxygen was given out from the water; no gas was given out from the acid, and it became green.

In the experiments on the sulphuric and nitric acids, it is most probable, that the acids were decomposed by the nascent hydrogen in the silver tube. In the experiments on the muriatic acid and ammoniac, the deficiency of the oxygen in the gold tube most probably, partly arose from the oxydation of the gold, in consequence of what may be called predisposing affinity. In these processes, none of the compound bodies appear to have been immediately decomposed by the galvanic influence.

The difference between my results and those of Mr. Henry, may be accounted for from the difference between our modes of operating; I suspect, however, that on repeating his experiment on the solution of pot-ash under new circumstances, that ingenious chemist will find reasons for altering his conclusions with regard to the decomposition of the alkali.

Judging



Judging from the rapidity with which the gases were extricated, solution of pot-ash would seem to be a better conductor than water, water a better conductor than solution of ammoniac, and solution of ammoniac, better than either of the three mineral acids. Possibly phosphoric acid, and other acids, may be decomposed when exposed to nascent hydrogen, produced under the galvanic influence. If the ratio between the quantities of the oxygen and hydrogen produced from the different wires, be always the same, whatever substances are held in solution by the water connected with them, this nascent hydrogen will become a powerful and accurate instrument of analysis.

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### VIII.

*Description of the Machine for kneading Dough, which is made Use of in the Public Bake-Houses of Genoa; from a Model presented to the Patriotic Society of Milan\*.*

THE drawings in Plate XII. represent the machine for kneading; the parts being regularly numbered. 1. A wooden frame upon which the gudgeon of the arbor of the wheel is supported. 2. A strong wall of three and a half palms † thick, through which the arbor passes. 3. Another wall of the same kind at the distance of 21 palms. 4. The arbor, 30 palms long, and  $1\frac{1}{2}$  palms thick. 5. A large wheel turning on the arbor. Its diameter is 28 palms, and its width for admitting two men is 5 palms. 6. Steps within the wheel to afford foot hole for the men. They are  $\frac{1}{2}$  of a palm high, and 2 palms asunder. 7. A concrete cog wheel near the end of the arbor, and of the diameter  $12\frac{1}{2}$  palms. 8. A bar or beam of wood parallel to the arbor, proceeding from one wall to the other, 21 palms in length, and  $1\frac{1}{2}$  in thickness. 9. Another beam of the same dimensions not visible, because on the opposite side of the arbor; but its relative situation may be seen in the shadow upon the wall. 10. A cross piece near the wall 3, which connects the two pieces 8 together, and supports the extremity of the arbor. Its length is 14 palms, and its thickness  $1\frac{1}{2}$ . 11. Another cross piece of the same description near the first wall, but not visible on account of the interposition of that wall. 12. A strong curved piece attached to the pieces 8, 9. It receives the pivot of the upright arbor, which carries the lantern pinion and kneaders. Its clear length is 14 palms, and its thickness  $1\frac{1}{2}$  palms. 13. The lantern pinion; in diameter  $5\frac{1}{2}$  palms, and height  $1\frac{1}{4}$ . 14. The arbor to which is affixed a strong cross of wood. 15. The cross formed of two bars of wood unequally divided, in such a manner as that the four branches are of unequal lengths. They are 1 palm wide, and  $\frac{7}{12}$  of a palm thick. 16. Four wooden pieces or kneaders of a triangular form, fixed

\* From the Italian of the *Atti della Società Patriottica di Milano*, Vol. II.

† The Genoese palm, which is stated by Dodson, in his *Calculator*, at 9,780, Paris fcs. inches of our measure.

in each branch of the cross in its lower face, so that they revolve in the trough parallel to the arbor, with one of their edges foremost. They are  $1\frac{3}{4}$  palms long, and  $\frac{1}{2}$  a palm thick.

17. A circular trough, or barrel, made very strong, and well hooped with iron. Its diameter is 6 palms, its clear height  $1\frac{1}{2}$  palms, and the thickness of the wood  $\frac{1}{4}$  of a palm.

The leaven or mixture required to be kneaded, is made in a wooden trough 4 palms long, and 3 palms by the side in which it is carried to the kneading mill.

The action of this mill is easily understood. When the men walk in the large wheel, they turn the kneaders which divide and agitate the paste in the trough, and perform the same effect as is commonly produced by human hands. The trough contains 18 rubbi ( ) of flour, which being properly mixed and raised, is carried to the mill to be kneaded, where in a quarter of an hour it is made into excellent paste. But the state of the dough determines the proper time for taking it out, according to the judgment of the attendant baker; so that it may take a little more or less time, according to circumstances.

## IX.

*On the Use of Caoutchouc in Manufactures, and on the Amelioration of Spirits by Age. By J. A.*

TO MR. NICHOLSON.

SIR,

*Glyde, August 19, 1800.*

TO observe your attention to the useful parts, in preference to the mere curiosities of philosophy, has given me, in common with many others, great satisfaction: and I sincerely hope your labours are proving useful to yourself.

In No. 37, of your instructive Journal, I observe that Mr. Howitson, an old acquaintance of mine, has made several promising essays towards obtaining a valuable manufacture by covering cloth with the East Indian caoutchouc. In confirmation of the practicability of this, I have to mention, that I have had an opportunity of knowing that hats have been covered with it in South America. Capotas (a kind of cloaks common to the Portuguese and Spaniards) have also been covered with this substance; and I have known capotas made in Portugal, and sent to Brazil for this express purpose. It is evident that any article of dress covered with caoutchouc must be perfectly impervious to wet; but the weight of the capotas (which it was said the manufacturers could not remedy) was so enormous, that they could only be serviceable as boat cloaks.

I have often remarked that spirits, particularly West India rum, is universally admitted to improve as its age encreases; but I do not find that this has been completely and philosophically accounted for: therefore, through the medium of your monthly publication, I should be glad to know the result of any research that may have been made into the



nature of this change. Your observations on this subject, and perhaps those of some of your correspondents who have directed their attention this way, cannot fail to be very interesting and useful to the West India merchants, and will be highly gratifying to

SIR,

Your humble servant,

J. A.

P. S. As exposure to the atmosphere has been found to produce a premature old age in rum, it has been presumed that it was caused by evaporation; but if this was the case, reducing the strength of the spirit with water ought to have an effect nearly similar. Whatever may be lost by exposure, I have formed the idea, and am clear in it (though I have only the vague evidence of the senses, without any chemical experiment on my side) that a great part of the improvement so observable arises from some absorption. At first I thought oxygen was absorbed; but the increase of a certain oily appearance does away this idea. It has also occurred that (agitation promoting the improvement) the particles of liquor may undergo some more intimate combination in consequence of something similar to friction. The particles of old liquor having been kept long in close contact, may have been by that means more assimilated. These ideas are, however, quite crude, and deserve little attention.

The changes which take place in ardent spirits and the other products of fermentation by keeping, have not formed the subject of direct investigation. How far the slow re-action of principles, the extrication of essential oil by subsidence or floating, the absorption or combination oxygen from without, or of tannin or gallic acid from the cask, &c. may be concerned, can only be collected from enquiry among intelligent dealers in these articles. I shall take the first opportunities of doing this, and stating the results.—W. N.

## X.

*Abstract of a Memoir on a Method of using the Syphon for raising Water in the Machine of C. Trouville. By Citizen PRONY\*.*

**T**O a tube A B (Plate IX. fig. 4.) of any form, are adapted a number of vertical branches; one of them terminates in a reservoir E, filled with water, and distinguished by the name of the great aspirator, and the others lead to reservoirs C, full of air, named small aspirators. Each small aspirator communicates by its tube, supposed to be filled with a

\* Bulletin de la Société Philomatique, II. 92.

reservoir beneath, which is open, and filled with water; and there is a valve opening upwards at the lower end of each of these tubes. The air within is of the same density and temperature as that without; but the external communication of this included air is prevented by the sides of the engine and the water which occupies part of its interior.

The great aspirator, the small aspirators, and the reservoirs, placed beneath these last, have all the form of vertical prisms; the small aspirators are equal and similar, and the portions of tube comprized between the aspirators and their reservoirs are of equal length.

Things being in this situation, we are to suppose that the water of the great aspirator flows out by the orifice  $f$ , and it is required, 1. what will be the depression of the water when the spring of the included air, and the charge of the water on the orifice of emission, shall be equal to the weight of the atmosphere; 2. the quantity of water which, at this period, shall have risen in the small aspirators; 3. the requisite dimensions of the apparatus, in order that the water emitted from the great aspirator, plus that raised in one of the other aspirators may be a determinate part of the water which a spring of known product affords in a day; and, 4. the proportions of the same apparatus which shall give the greatest ratio between the sum of the volumes raised in all the small aspirators, multiplied by their elevation, and the volume of water emitted from the great aspirator, multiplied by its height.

*Formula for the Solution of the foregoing Problem.*

Let  $Q$  = the volume of water afforded by the spring in one day.

$\gamma$  = the number of times which this volume of water contains that which flows from the great aspirator from the instant of its being filled to the cessation of its efflux.

$c$  = the height of the great aspirator.

$b$  = the common length of the portion of pipe comprized between a small aspirator and the reservoir, whence its water is drawn.

$n$  = the number of small aspirators.

$s$  = the horizontal section of each small aspirator.

$t$  = the horizontal section of the lower reservoir, from which the small aspirator draws the water.

$S$  = the horizontal section of the great reservoir.

$V$  = the capacity of the interior parts of the machine, which are at no time occupied by water, but constantly hold air.

$\lambda$  = the height of the column of water which measures the total pressure of the atmosphere.

$x$  = height of the water above the orifice of emission of the great aspirator, at the moment when the spring of dilated air within, and the charge of water on the orifice of emission, are equal to the weight of the atmosphere.

$y$  = the correspondent depression of the upper surface of the water in the great aspirator.

$z$  =



$\kappa$  = the correspondent height of the prism of water, which penetrates into any one of the small aspirators.

Making  $\frac{\lambda}{s} = K$ ;  $K + 1 = m$ ;  $V m + S m (\lambda + c) + n s \lambda = A$ , we have

$$\kappa = \frac{A \pm \sqrt{A^2 - 4 S m \lambda (n h s + S m c)}}{2 S m};$$

and assuming the equation  $\psi = \frac{\lambda}{\lambda - \kappa}$ , we obtain the following:

$$\begin{aligned} y &= c - \kappa = \frac{\lambda - (\lambda - c) \psi}{\psi}, \\ z &= \frac{\kappa - b}{m} = \frac{\lambda (\psi - 1) - b \psi}{m \psi}, \\ \psi &= \frac{\lambda}{\lambda - b - m z} = \frac{\lambda}{\lambda - c + y}, \\ b &= \frac{(\lambda - m z) \psi - \lambda}{\psi} \end{aligned}$$

which relates to the vertical dimensions. With regard to the horizontal, making  $\alpha = \frac{V}{n s z}$ , we have,

$$\begin{aligned} s &= \frac{Q - \gamma S y}{\gamma z} = \frac{Q}{\gamma z (1 + n [a (\psi - 1) + \psi])}, \\ S &= \frac{Q - \gamma s z}{\gamma y} = \frac{z}{y} \cdot n s (\alpha (\psi - 1) + \psi). \end{aligned}$$

Lastly, if we suppose the prisms which we have called great aspirators, to have their height = the value of  $z$  here found; the ratio between the product of the height of the great aspirator by its expenditure, and the product of the mass of the water raised by the height to which it is raised, namely,  $\frac{c}{b} \cdot \frac{S y + s z}{n s z}$ , being denoted by  $\Phi$ , we have the formula.

$$\Phi = \frac{c}{b} \left\{ a (\psi - 1) + \psi + \frac{1}{n} \right\}.$$

If from  $\Phi$  the term  $\frac{c}{n b}$  be retrenched, the surplus will be the ratio between the moving cause and the effect produced; and the less this ratio differs from unity, the more will the effect approach the maximum of value of which it is capable. It is requisite, in order that this maximum of value should take place, 1. that the excess of  $m$  beyond unity, and the height should be diminished, as much as the possibility of execution will allow; and, 2. that  $\psi$  should be determined by the equation.

$$\psi = \frac{\lambda}{\lambda - m z} \left\{ 1 \pm \sqrt{\frac{\lambda + a m z}{(a + 1) \lambda}} \right\}.$$

The

The theory of the machine of Cit. Trouville is deduced from the preceding formula, supposing the spring to have a fall equal to the height of the great aspirator, to which it supplies water; and that the first small aspirator draws from the spring, and empties itself into the reservoir of the second aspirator; that this discharges into the reservoir of the third, and so forth; all these effects producing each other by appropriate machinery.

## SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

### *Klaproth's Analytical Essays.*

IT will undoubtedly afford much satisfaction to the cultivators of philosophical chemistry, to be informed, that a translation of the above work, by the learned editor of *Gren's Principles of Modern Chemistry*, is in the press, and will shortly appear.

### *Multiplication of Engravings in Relief.*

Mr. George Gray, of Newcastle upon Tyne, has presented me with some specimens of a very useful application, of the art of taking impressions in soil, or leaf metal. They consist of three blocks; the first about three inches square, containing sixteen lines of pica italic, formed by impressing a leaf of tin foil upon the face of the printers types, set up in the usual manner. The tin foil is strengthened by plaister of Paris poured into the concavity, to the thickness of a little more than half an inch. This block is not as sharp as the type itself; but affords impressions on paper perfectly legible, and about as neat as common ballads are usually printed. The second is an impression taken in brass foil, from a wood cut of *Bewicke*,  $2\frac{1}{4}$  inches long, and near  $1\frac{1}{2}$  inch broad. A solution of sal ammoniac was applied to the concave side, and lead then poured in, which consequently adhered, and gave the requisite strength. It is very sharp, but the face has been bended in taking off; so that it is not easy to print from it. The third is a small figure of a ship, within a bounding line of three quarters of an inch square. It is made in the same manner with brass foil, and though the engraving abounds with fine strokes, it affords an impression which could not, I think, be distinguished from another taken from the original.

It may perhaps be true, that this art is rather inferior to the method of moulding and casting, described at page 63 of the second Volume of this Journal. But it requires less of preparation, as well as of skill, and with some improvements in the soil, the manipulations and the strengthening, may probably exceed the other method in precision and effect.

*Rational*



*Rational Toys.*

I give the following from a paper transmitted to me :

It requires but slight observation to be satisfied of the utter inutility of the articles with which the Toy-shop is usually replenished. And on close reflection, it may possibly appear neither strained nor severe to condemn most play-things as worse than useless. Their gay appearance, and the movements which they are sometimes contrived to perform, doubtless raise strong and sudden desires in the mind of children. But satiety as quickly follows. The flush of delight, arising from the first impression, cannot but be transitory; and no sooner does the little possessor examine into the structure of his new acquisition, than he flings it aside, or breaks it to pieces and tramples it under foot, as if to revenge himself upon it for belying the promise of its exterior.

This succession of longing and loathing is a more serious evil than may at first be apprehended. If it be true, that youthful curiosity cannot be frequently balked with impunity, every such disappointment may be considered as some advance towards dullness.

We often meet with a species of toy calculated to excite surprise. This, if not liable to the same objection as the unmeaning toy, may be suspected of fostering a disposition for petty stratagems, by which a connection between pleasure in the individual who plays them off, and pain in others is almost inevitably established.

Ten years ago, the idea of substituting models of machines in the place of ordinary toys, suggested itself to one of the persons whose names are subjoined to the present paper.

Every quality, he conceived, which distinguishes models, would secure them against neglect and destruction, the merited fate of toys. The knowledge conveyed by the mutual dependency of the parts, and by the purpose of the whole, would be laid up with advantage and might be revived with pleasure. Whatever improvement the understanding derives from mathematics, would more agreeably flow from well-constructed models. And mathematics would be studied with more success by children accustomed to such models. They would rouse the faculty of invention, and confer the habit of pursuing trains of thought to a great extent. To girls, by conveying information without awakening their sensibility, they would be particularly serviceable. From this statement, the utility of a set of models in schools and private families is obvious. It is equally obvious that their utility would not be confined to young people.

This scheme has been generally approved by those to whom it has been mentioned. Different persons, long since, offered to advance money towards its execution. Indeed, in 1796, it was partially carried into execution, when the whole design was announced under the title of Rational Toys, in a letter prefixed to Mr. Donne's explanation of his elementary mathematical models. Towards its complete execution, however, there was wanting a person well informed concerning machines, and of ready mechanical invention. This difficulty is now removed by the offer of Mr. Robert Weldon, to conduct a manufactory of Rational Toys.

It is however, requisite that a sum of money sufficient to set it on foot be advanced; and to satisfy subscribers (as far as such a circumstance may afford satisfaction) that they are likely

likely to receive an adequate return for any sum they may advance, and also out of regard to Mr. Weldon, the persons, named below, have agreed to act as a committee of superintendence.

Soon after a sufficient sum shall have been subscribed, Mr. Weldon will furnish the subscribers with a list of models and prices, from which each may choose to the amount of his subscription. A description and drawing will accompany the models; and they will be contrived so as to take to pieces, and its name will be fixed to each part, where this is practicable.

Hereafter, if the undertaking prospers, the public may be accommodated with single models. But at present to spare the committee's time, it is proposed:

I. That no subscription of less than 10l. be received.

II. That every subscription not exceeding 10l. be immediately advanced. Larger subscriptions may be paid by instalments of 10l.

These terms may startle many, even opulent, parents. And it is true that a set of instructive models will require a greater sacrifice than has been customary in education. But as public opinion on this subject is daily becoming more liberal, it may begin to be felt that of all possible methods of consulting happiness, to subtract information for the sake of adding to fortune, is the most preposterous. A few additional hundred pounds, well laid out upon the boy, may more improve the condition of existence than the mines of Potosi, bequeathed to the man. The ill-advised niggardliness of parents is one principal cause why genius has so seldom been exerted upon the means of early instruction, and why the most difficult of all occupations—that of forming the system of thought and feeling—has been so often confided to persons, whose endowments are barely adequate to the rudest mechanical labour.

#### COMMITTEE.

THOMAS BEDDOES, M.D. Clifton.

JOHN BILLINGSLEY, Esq. Ashwick-Grove.

WM. CLAYFIELD, Esq. Bristol.

BENJ. HOBHOUSE, Esq. M.P.

JOHN H. MOGGRIDGE, Esq. Boyce-House.

JAMES STEPHENS, Esq. Camerton-House.

JOHN WEDGWOOD, Esq. Cote-House.

WM. WYNCH, Esq. London.

Subscriptions are received by

SAVERY and Co. Bankers, Bristol.

HOBHOUSE and Co. Bankers, Bath.

WM. REYNOLDS and Co. Bankers, Salop.

SIR JOHN CALL PYBUS, Bart. and Co. Bankers, London.

SIR JAMES ESDAILE and Co. Bankers, London.

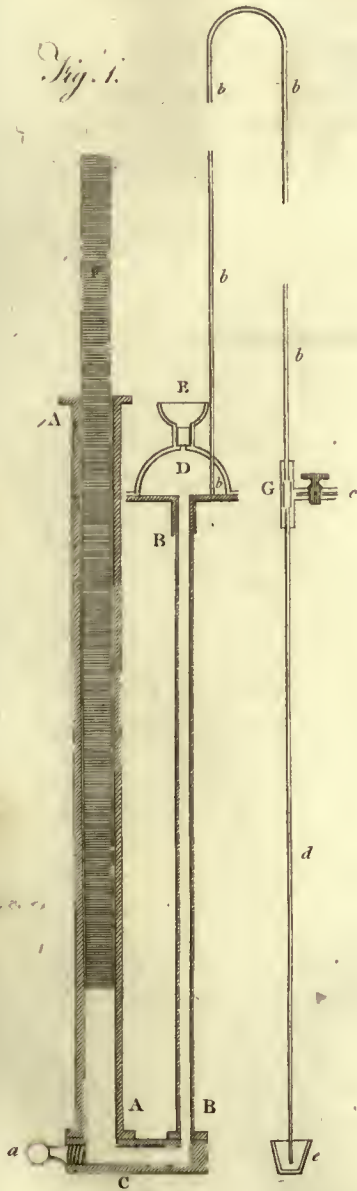
\* \* Letters (post-paid) addressed to Mr. WELDON, Pneumatic Institution, Bristol, will be duly attended to.



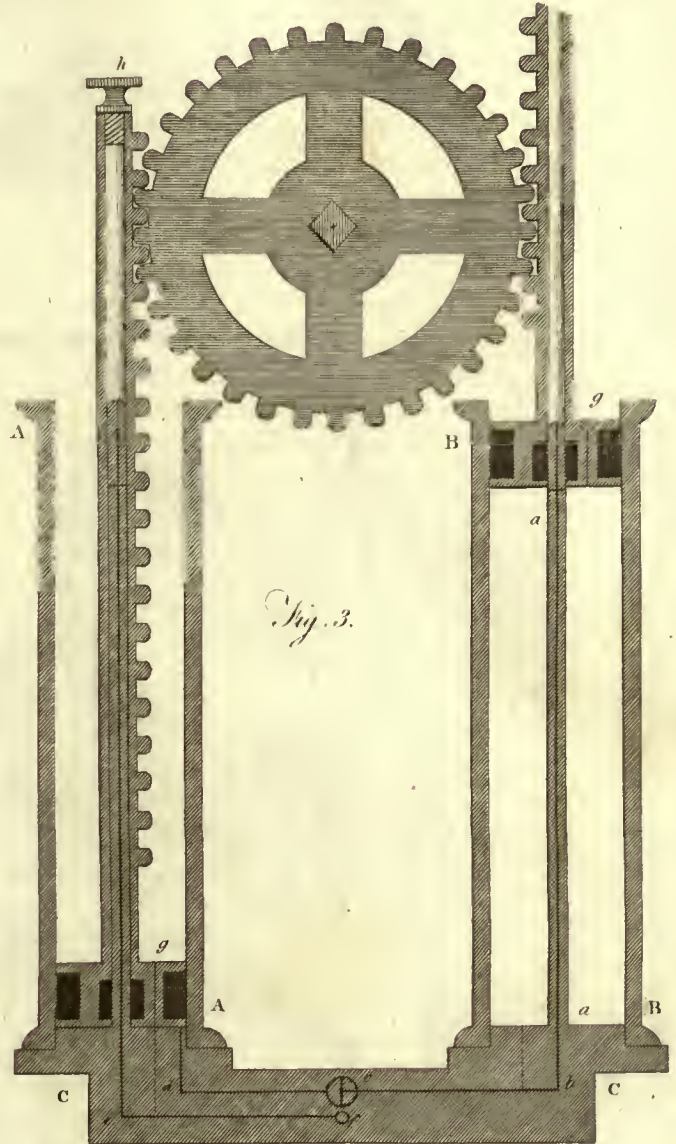
*Mercurial air pump.*

*Double barreled air pump.*

*Fig. 1.*

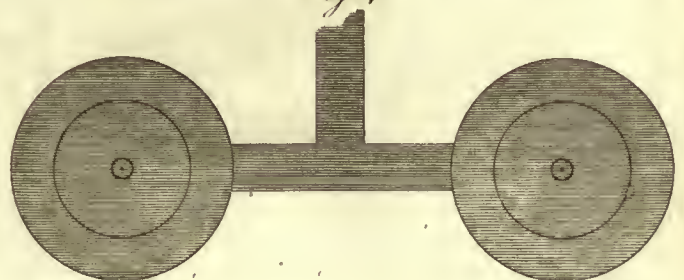


*Fig. 2.*



*Fig. 3.*

*Fig. 4.*

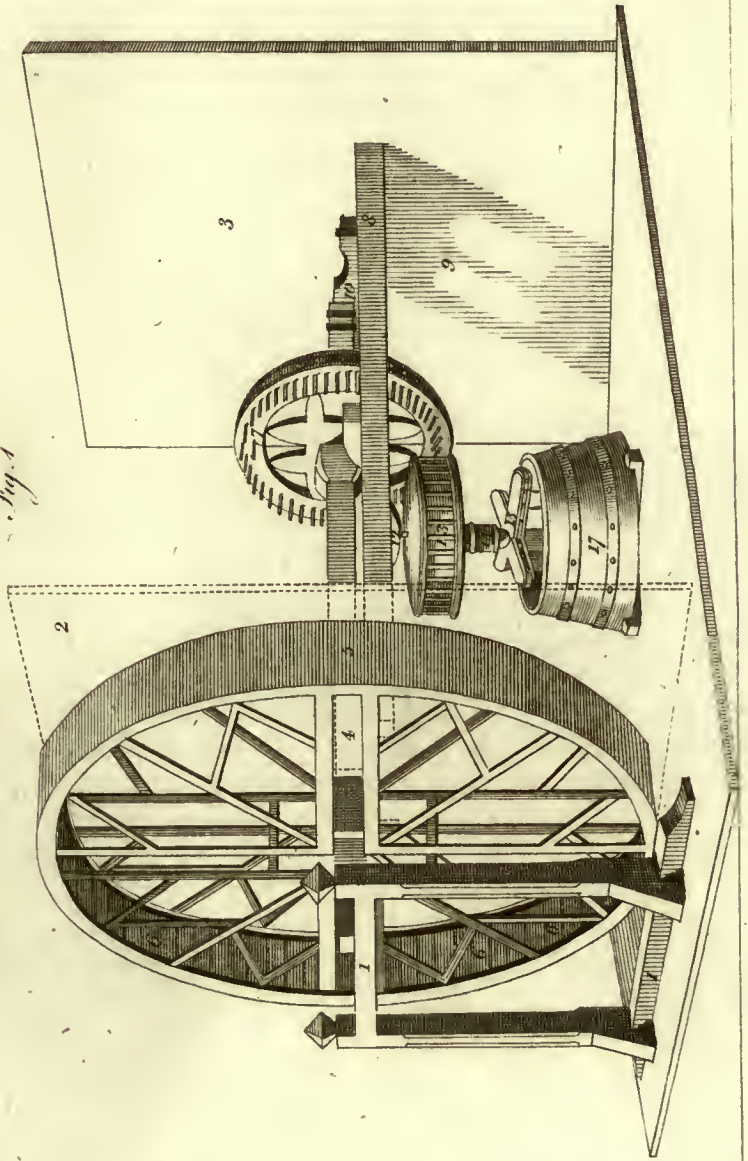




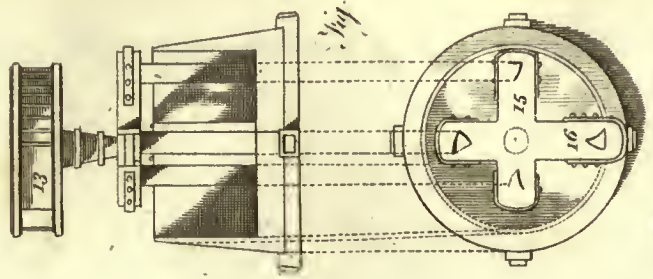


*Knending Machine.*

*Fig. 1*



*Fig. 2*







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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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OCTOBER 1800.

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ARTICLE I.

*A Project for extending the Breed of Fine-wooled Spanish Sheep, now in the Possession of his MAJESTY, into all Parts of Great Britain, where the Growth of fine Clothing Wools is found to be profitable\*.*

AFTER experiments had been tried for several years, by the King's command, with Spanish sheep of the true *Merino* breed, imported from various parts of Spain, all of which concurred in proving, that the valuable wool of those animals did not degenerate in any degree in this climate, and that the crosses of a *Merino* ram, uniformly increased the quantity and meliorated the quality of the wool of every kind of short-wooled sheep, on which it was tried, and more particularly so, in the case of the South Down, Hereford, and Devonshire breeds; his Majesty was pleased to command, that some *Merino* sheep should be procured from a flock, the character of which for a fine pile of wool, was well established.

Application was accordingly made to Lord Auckland, who had lately returned from an embassy to Spain; and in consequence of his lordship's letters, the Marchioness del

\* This paper was drawn up by the Right Hon. Sir Joseph Banks, Bart. P. R. S. &c. at whose expence it has been lately circulated.

Campo di Alange was induced to present to his Majesty five rams and thirty-five ewes, from her own flock, known by the name of *Negretti*, the reputation of which, for purity of blood and fineness of wool, is as high as any in Spain : for this present, his Majesty was pleased to send to the Marchioness in return eight fine English coach horses.

These sheep, which were imported in the year 1792, have formed the basis of a flock, now kept in the park of his Royal Highness the Duke of York at Oatlands, the breed of which has been preserved with the utmost care and attention.

The wool of this flock, as well as that of the sheep procured before from Spain, was acknowledged by the manufacturers who saw it, to be to all appearance of the very first quality, yet none of them chose to offer a price for it, at all equal to what they themselves gave for good Spanish wool, lest, as they said, it should not prove in manufacture so valuable as its appearance promised; it became necessary, therefore, that it should be manufactured at the King's expence, in order that absolute proof might be given of its actual fitness for the fabric of superfine broad cloth; and this was done year after year, in various manners, the cloth always proving excellent; yet the persons to whom the wool was offered for sale, still continued to undervalue it, being prepossessed with an opinion, that though it might not at first degenerate, it certainly sooner or later would alter its quality, much for the worse.

In 1796 it was resolved to sell the wool at the price that should be offered for it, in order that the manufacturers themselves might make trial of its quality, although a price equal to its real value should not be obtained; accordingly, the clip of that year was sold for 2s. a pound, and the clip of the year 1797, for 2s. 6d.

The value of the wool being now in some degree known, the clip of 1798 was washed in the Spanish manner, and it sold as follows:

The number of fleeces of ewes and wethers was 89;

Which produced in wool washed on the sheeps' backs - 295 lb.

Loss in scowering - - - - 92

Amount of scowered wool - - - - 203

Which produced, Raffenos, 167 lb. at 5s. per lb. }  
 Finos, 23, at 3s. 6d. } 47l. 8s.  
 Terceros, 13, at 2s. 6d. }

The clip of 1799 was managed in the same manner, and produced as follows:

The number of fleeces of ewes and wethers was 101;

Which produced in wool, washed on the sheeps' backs - 346 lb.

Loss in scowering - - - - 92

Amount of scowered wool - - - - 254

Which produced, Raffenos, 207 lb. at 5s. 6d. per lb. }  
 Finos, 28, at 3s. 6d. } 63l. 14s. 6d.  
 Terceros, 19, at 2s. }



The ram's wool of the two years sorted, together produced as follows :

Quantity of wool, washed on the sheeps' backs	-	314 lb.
Lofs in scowering	-	99
Amount of scowered wool	-	215

Which produced, Rassinos, 181 lb. at 4s. 6d. per lb.	}	45l. 15s. 6d.
Finos, 22, at 3s. 6d.		
Terceros, 12, at 2s.		

It is necessary to account for these extraordinary prices by stating, that in the year 1799, when both sales were effected, Spanish wool was dearer than it ever before was known to be; but it is also proper to add, that 5s. 6d. was then the price of the best Spanish piles, and that none were sold higher, except as is said, a very small quantity for 5s. 9d.

The King has been pleased to give away to different persons, who undertook to try experiments, by crossing other breeds of sheep with the Spanish, more than one hundred rams and some ewes: in order, however, to make the benefit of this valuable improvement, in the staple commodity of Great Britain, accessible to all persons who may choose to take the advantage of it, his Majesty is this year pleased to permit some rams and ewes to be sold, and also to command that reasonable prices shall be put upon them, according to the comparative value of each individual; in obedience to which it has been suggested, that five guineas may be considered as the medium price of a ram, and two guineas that of a ewe; a sum which it is believed the purchaser will in all cases be able to receive back with large profit, by the improvement his flock will derive from the valuable addition it will obtain.

Though the mutton of the Spanish sheep was always excellent, their carcases were extremely different in shape, from that mould which the fashion of the present day teaches us to prefer; great improvement has however been already made in this article, by a careful and attentive selection of such rams and ewes as appeared most likely to produce a comely progeny: and no doubt can be entertained, that in due time, with judicious management, carcases covered with superfine Spanish wool, may be brought into any shape, whatever it may be, to which the interest of the butcher, or the caprice of the breeder, may chuse to affix a particular value.

Sir Joseph Banks, who has the honor of being intrusted with the management of this business, will answer all letters on the subject of it, addressed to him in Soho-square. The rams will be delivered at Windsor, the ewes at Weybridge in Surrey, near Oatlands.

As those who have the care of his Majesty's Spanish flock, may naturally be supposed partial to the project of introducing superfine wool into these kingdoms, it has been thought proper to annex the following notice, in order to shew the opinion held of a similar undertaking, in a neighbouring country, where individuals, however they have mistaken their political interest, are rather remarkable for pursuing and thorough weigh-

ing their own personal advantage, in all their private undertakings, and for sagacity in seizing all opportunities of improving, by public establishment, the resources of their nation.

#### FRENCH ADVERTISEMENT.

On the 24th of May last, an advertisement appeared in the *Moniteur*, giving notice of a sale of two hundred and twenty ewes and rams of the finest woolled Spanish breed, part of the flock kept on the national farm of Rambouillet; also two thousand pounds of superfine wool, being the present year's clip of this national flock, and one thousand three hundred pounds of wool, the produce of the mixed breeds of sheep kept at the Menagerie at Versailles.

This advertisement, which is official, is accompanied by a notice from Lucien Buonaparte, Minister of the Interior, as follows:

"The Spanish breed of sheep that produce the finest wool, introduced into France thirty years ago, has not manifested the smallest symptom of degeneration: samples of the wool of this valuable flock, which was brought from Spain in the year 1786, are still preserved and bear testimony, that it has not in the least declined from its original excellence, although the district where these sheep have been kept, is not of the best quality for sheep farming; the draughts from this flock, that have been annually sold by auction, have always exceeded in value the expectation of the purchasers, in every country to which they have been carried, that is not too damp for sheep.

"The weight of their fleeces is from six \* to twelve pounds each, and those of the rams are sometimes heavier.

"Sheep of the ordinary coarse woolled breeds, when crossed by a Spanish ram, produce fleeces double in weight, and far more valuable, than those of their dams; and if this cross is carefully continued, by supplying rams of the pure Spanish blood, the wool of the third or the fourth generation, is scarce distinguishable from the original Spanish wool.

"These mixed breeds are more easily maintained, and can be fattened at as small an expence, as the ordinary breeds of the country.

"No speculation whatever offers advantages so certain, and so considerable to those who embark in it, as that of the improvement of wool, by the introduction of rams and ewes of the true Spanish race, among the flocks of France, whether the sheep are purchased at Rambouillet, or elsewhere; in this business, however, it is of the greatest importance to secure the Spanish breed unmixed, and the utmost precaution on that head should be used, as the avarice of proprietors may tempt them to substitute the crossed breeds instead of the pure one, to the great disappointment of the purchaser.

\* This must mean fleeces unwashed, or in the yoke, as it is technically termed.



"The amelioration of wool at Rambouillet has made so great a progress, that in a circle from twenty-four to thirty-six miles in diameter, the manufacturers purchase thirty-five thousand pounds of wool, improved by two, three, or four crosses. Those who wish to accelerate the amelioration of their flocks by introducing into them ewes of this improved sort, may find abundance to be purchased in that neighbourhood at reasonable rates."

## II.

*Description of an Engine for raising Water by the lateral Motion of a Stream of Water through a conical Tube In two Letters from Mr. WILLIAM CLOSE.*

## LETTER I.

TO MR. NICHOLSON.

S I R,

Dalton, Aug. 25, 1800.

PART of this town being situated upon a dry eminence of lime-stone, the inhabitants experience considerable fatigue and inconvenience in providing the quantity of water which is indispensibly necessary for culinary and domestic purposes. There are excellent springs in the neighbourhood, and at a small distance, if taken in a direct line, but the nearest road to return is steep and incommodious. This situation has frequently turned my attention to the contemplation of hydraulic machinery to remove the inconvenience, and I doubt not it might easily be done by various methods, as a stream of considerable force runs very near the springs, and the height to which water might be raised with considerable advantage is not great: but this is not the object of my communication.

The experiments of Citizen Venturi on the lateral motion of fluids, induce me to think that an hydraulic engine, operating by the pressure of the atmosphere, and in this respect similar to that excellent one invented by H. Goodwin, Esq.\* may be brought into action by this principle. With this view I have made some experiments, and endeavoured to reduce my ideas into the form of an engine, which I expect to see much improved, and of which I send you a drawing and description.

A A, in Plate XIII. Fig. 1, represents a reservoir of water, kept constantly full, at the same time that the jet B is running full under a considerable pressure.

C a tube fixed into the bottom of the spherical vessel D, and passing up to some height above its middle.

E another tube fixed into the bottom of D, and ending near its top. The lower part of this tube is bended, and the extremity of it is introduced into the smaller aperture of the conical tube B.

F a pipe to empty the vessel D, when filled with water.

\* Philof. Journal, IV. 163.

G a small tube passing through the tube F, and rising to near the top of D, for admission of air to quicken the descent of water out of that vessel. Both these tubes are closed at their lower ends by the lever L, which is fixed upon a cock in the tube E, and has a weight upon one end, in order that the other end may bear against the openings of the tubes, at the same time that it supports the vessels I, which is suspended upon it by a cord or wire. The upper surface of that end of the lever which comes in contact with the tubes, must be covered with leather, and must press against them with considerable force, that the vessel D may be perfectly tight when the ends of the other two tubes are placed in water.

H a small cistern, provided with a syphon, to be filled with water from the reservoir A, at the same time with D; this must be done by regulating the cock *k*, upon the pipe which supplies H with water.

I a small vessel and syphon suspended from the lever L.

The operation of the engine will be as follows:

The reservoir A being constantly full of water, and the conical tube B completely filled at its wider end by the water which runs out of A; the force of the lateral motion being increased by the form of the tube B, and acting upon the end of E, will rarify the air in D, and the pressure of the atmosphere upon the surface of the water in the reservoir, will cause part of that water to rise in the tube C to run over its top and fill D, then to descend through E, and enter the stream which flows out at B.

When D is full of water, if the tubes F and G are opened, the water will run out; therefore, soon after D is full, H is full also, and the syphon of this last vessel beginning to empty the water, fills I, which overbalances the weight upon the lever L, and opens the pipes F and G, and closes the cock in E: the column of water in C immediately descends into the reservoir, and if the small tube G be full of water, it will be emptied by the descent of that column, admit air into D, and the water will flow out at F. The vessels D and H will be empty of water about the same time, and the vessel I, by its syphon soon after: the weight upon the lever L will then close the tubes F and G, open the passage through E, and the water will rise into D as before.

If the water should descend through E, before F and G are opened, it will render the cock in E more tight.

*Experiment 1.* The vessel D was constructed of a cylindrical form, one inch and three quarters in diameter, and two inches in height, without the tubes F and G, but with two holes, one to let out water, and the other to admit air, both closed with wooden stoppers. The tube C was three-eighths of an inch inside diameter, and the tube E, one-fifth of an inch in its internal diameter: this last was placed within the tube C for readiness of construction, its upper end was slanted off, and touched the top of D, and it was confined in its place by a wedge of wood thrust into the bottom of C. A conical tube, nine inches long, one inch diameter at its smaller end, and one inch and three quarters at its wider end, was fixed to the side of a reservoir eight inches deep. The depth of the water in the reservoir



reservoir above the smaller aperture of the conical tube being six inches and a half, and the water which ran through that tube completely filling its wider extremity; the crooked end of the tube E was introduced into the smaller aperture of the conical tube, while the bottom of C was two inches below the surface of the water in the reservoir; part of the water immediately rose fourteen inches in C and filled D; the tubes C and E were then lifted out of the water, and D was emptied by taking out the stoppers. The experiment was frequently repeated.

*Experiment 2.* The vessel D was made spherical, two inches diameter, the same tubes C and E adapted as before. The two tubes F and G were added to this, and closed by the finger of the operator when water was raised. The tube E being lengthened a little, was placed within the small aperture of B, when the charge of the reservoir was seven inches. The bottom of the tube C was just below the surface of the water. In a very short time part of the water rose near sixteen inches through C, and filled D; the tubes *f* and *g* were then opened, and this water ran out.

From several experiments, I think water will not rise more than sixteen inches under similar circumstances.

Upon the admission of air into D, the stream of the jet diminished so much, that it did not half fill the wider aperture, and it was long after the tubes F and G were closed before it could recover its full force. This shews the necessity of a cock or valve to close E, to prevent the diminution of the jet, when air is admitted into D, if we wish to make the engine raise much water, and empty itself at stated times. For no water will rise so long as the conical tube is not full. If a cock be perfectly air tight, it will answer the purpose very well.

Fig. 2 points out a method of constructing a valve for this purpose: the vessel D, in this figure, is cylindrical, the small tube *g* opens very near the top, under that letter, and the dotted line *n* passes through the middle of F. The tube C and E are explained before. *m* is a valve which, by its weight, closes the upper aperture of E, when the lever L opens the tubes F and *g*, and which immediately before it closes those pipes it raises again: the communication may be made by a piece of strong wire fixed into *m*, and which, passing through *f*, will rest upon the lower valve or lever L. This wire is represented by the dotted line *n*.

It perhaps might be advantageous to have a valve in C to support the column of water in that tube also.

The power of the engine seems to be very considerable when constructed upon the simple plan mentioned in the experiments; it seems very desirable to determine the best form of the conical tube, and the method to throw most force upon the rarifying tube, so as to produce the greatest effect under a given pressure of water in the reservoir. Citizen Venturi has determined the form of a tube to produce the greatest expenditure\*, and it seems likely that it may also be the best form for this purpose.

\* Philos. Journal, II. 274. This figure is adapted to a certain definite pressure only, and must be varied for other depths.---N.

The lateral motion of a current of air will, to a certain extent, produce the same effect as a current of water, but to what height water may be raised in this way, I have hitherto made no experiment to determine. The power of an instrument in the form of Mr. Boswell's ventilator \* is very considerable when made upon a small scale, if the small tube be forcibly blown through while the end of the vertical tube is placed in water, one expiration will cause the water to rise in it several inches.

I am, SIR,

Your's, &c.

WILLIAM CLOSE.

## LETTER II.

SIR,

HAVING constructed the model of an engine upon the preceding principles, on the 29th of August last, I set it to work, and kept it in action for several hours.

The rarefying tube E, fig. 1. was placed within the ascending water tube C, until they had both entered the spherical vessel; they were then divided, and the end of the water tube was used as the support of a metallic valve, which, by its weight, closed the opening of the tube E. To one side of this last tube was fixed another small one, which opened into the external air by the side of the pipe to let out water, and both these were closed by the end of the lever L, or lower valve. This small tube, at the same time that it served for admission of air, to hasten the descent of water, served also to contain the wire which raised the upper valve, n, fig. 2.

The lower valve was covered with leather, and when it was well moistened, and the engine had been in action for some time, I could perceive no fault.

The method of making the engine work itself, which I adopted in imitation of the plan proposed by Mr. Boswell †, answered very well: the lower valve was opened between six and seven times in a minute, and water always flowed out; the higher valve completely prevented the diminution of the stream through the conical tube, and when it was lifted by the closing of the lower valve, no difference could be perceived, although a little air was admitted at each rise.

The descending branch of the syphon in the higher vessel H, was made of considerable length, to prevent a constant dribble, and to make the reciprocation end at once. The syphon of the lower or descending vessel was made full as wide as the other, in order that the weight might preponderate quickly, and close the lower valve immediately after raising the higher. The upper vessel was hung upon the side of the reservoir, and supplied with water over the edge by a syphon, one aperture of which was made of a proper width. The capacity of the lower vessel was about five ounces; the higher about seven; but

\* Philos. Journal, IV. 4.

† Philos. Journal, IV. 117.



was sometimes diminished, by introducing a solid body into its upper part, to make it fill sooner. Weight, rather above six ounces. Length of lever, one foot; it had holes in its under edge to hang the vessel and weight at different distances from its centre. The rise of the water above the surface of water in the reservoir was one foot. I adopted this height for convenience. There was no valve to prevent the column from descending every time the lower valve was opened.

This particular description may appear tedious, but it will save some time and consideration in the repetition of such an experiment.

I entertain no doubt respecting the operation of an engine of this kind, and that a column of water may be raised to any height, not much exceeding thirty feet, by proportionally encreasing the pressure of water in the reservoir, and the dimensions of the conical tube: in many situations, however, the requisite quantity for this purpose cannot be had, others may not admit of sufficient descent, and in some a reservoir so deep may be dangerous and inconvenient.

Where the stream has sufficient descent, the water may be raised by a number of *lifts* instead of one.

Suppose 1, 3, 5, fig. 3. to be three reservoirs with conical tubes, and 2, 4, 6, three cisterns, through which the raised water is carried in succession by a stream passing through the three reservoirs, and acting upon three engines in the following manner:

The cisterns 2 and 4 being filled with water, the current through the reservoir 1 will raise a quantity of water into its engine, which will be emptied into the cistern 2, at the same time the reservoir 3 raises an equal quantity out of 2 into 4, and 5 another out of 4 into 6.

The same method may be used when there is plenty of water, but not convenience for a deep reservoir; in this case the conical tubes may be fixed to different parts of the reservoir, and all upon the same level. Each engine must be provided with a lever and weight, to work its own valves; but they may be all opened at the same time by the descent of one vessel connected to all the levers, or each by a vessel connected to that only.

To quicken the reciprocations of the engine, and encrease the quantity of raised water, a valve may be made to support the column of water in the ascending tube; if the column be not heavy, this valve may be contained in a chest at the bottom of the tube: it will be easy to repair in this place; a piece containing it may be fixed on with screws: it will rise by the pressure of the water below.

This kind of machinery, by altering the position of the rarefying tubes, will raise water from a depth below the stream equally as well as to an height above it; and in situations where there is plenty of water and convenience for a reservoir, a lower body of water may be conveyed into a stream above by the help of a single tube only; one end of it being placed in the water to be raised, the other must be introduced into the smaller

aperture of the conical tube adapted to the reservoir. A constant stream will then rise so long as water below can supply the tube\*.

I am,

SIR,

Your humble servant,

Dalton, Sept. 1, 1800.

W. CLOSE.

My acknowledgements are due to Mr. Close for his obliging private postscript. In reply to his doubt, left from the number of hydraulic inventions which have appeared in this Journal, his contrivance might not be inserted; it seems proper to observe, that I consider hydraulic works as of the highest importance to society, whether we look to a supply of the first necessity for consumption, or to the advantageous uses of neglected, though valuable, first movers; that these works must in most cases be modified by localities and other circumstances; and, consequently, that the most useful practical knowledge will not consist in an acquaintance with one or more of the best engines, but with that great variety of happy contrivances which reading and enquiry must point out. From reasons of this sort, when exercising my duty as editor of a work rendered respectable by the correspondence and favor of learned and ingenious men, I have been little solicitous of the abstract comparative merits of the works I receive. If they contain useful facts, clear deductions, or new prospects tending to lead others to improvements in science or the arts, they are certainly well calculated to form part of this miscellany.—N.

### III.

*On double Images caused by Atmospheric Refraction.* By WILLIAM HYDE WOL-  
LASTON, M. D. F. R. S.†.

IN some of the last volumes of the Philosophical Transactions, there have been related many instances of strong atmospheric refraction, by which objects seen near the horizon have appeared inverted, and the horizon itself either elevated or depressed.

Mr. Huddart first took notice of a distant image, inverted beneath the object itself; and, in the Philosophical Transactions for 1797‡, has described several such appearances, accompanied with an optical explanation, wherein he shews, that the lowest strata of the air were at the time endued with a weaker refractive power, than others at a small elevation.

\* See the experiments of Venturi on the lateral motion of fluids, before quoted.—C.

† Phil. Transf. 1800, p. 239.

‡ On this Journal, I. 146. See also 223, and elsewhere, as by the index.



In the volume for 1799, Mr. Vince has given an instance\* (tab. 1. fig. 1.) where erect as well as inverted images were visible above, instead of beneath, the objects themselves; and by tracing the progress of the rays of light, in a manner similar to Mr. Huddart's, concludes that these phenomena arose from "unusual variations" of increasing density in the lower strata of the atmosphere.

In the volume for 1795, Mr. Dalby mentions having seen "the top of a hill appear detached, for the sky was seen under it." In this case, as well as in the preceding, it is probable that reversion took place, and that the lower half of the portion detached was an inverted image of the upper, as the sky could not be seen beneath it, but by an inverted course of the rays.

Since the causes of these peculiarities of terrestrial refraction have not received so full an explanation as might be wished, I have endeavoured,

1st. To investigate theoretically the successive variations of increasing or decreasing density to which fluids in general are liable, and the laws of the refractions occasioned by them.

2dly. To illustrate and confirm the truth of this theory, by experiments with fluids of known density.

And lastly, to ascertain, by trial upon the air itself, the causes and extent of those variations of its refractive density, on which the inversions of objects and other phenomena observed, appear to depend.

The general laws may be comprized in three propositions.

*Prop. 1.* If the density of any medium varies by parallel indefinitely thin strata, any rays of light moving through it *in the direction of* the strata, will be made to deviate during their passage, and their deviations will be in proportion to the increments of density where they pass.

For each ray will be bent towards the denser strata, by a refracting force proportioned to the difference of the densities above and below the line of its passage; and as their velocities are the same, and therefore the times of action of the forces equal, the deviations will be at the refracting forces, *i. e.* as the increments of density.

*Prop. 2.* When two fluids of unequal density are brought into contact, and unite by mutual penetration; if the densities at different heights be expressed by ordinates, the curve which terminates these ordinates will have a point of contrary flexure.

For the straight lines  $da$ ,  $rn$  (Plate XIII.) Fig. 1. which terminate the ordinates  $rx$ ,  $dy$ , of uniform density, will be parallel, and, if not united by contrary curvature, some straight line of union, as  $ao$  must be supposed. But, from whatever cause the first line  $ao$  is inferred, by the same cause other intermediate lines  $mp$ ,  $tq$ , &c. will be produced, and curves  $defm$ ,  $mtsr$ , will be ultimately formed, having a point of contrary flexure at  $m$ .

The form of the curve does not appear to admit of accurate investigation, nor is it of importance to the subsequent reasoning, if wholly unknown. We may, however, form

\* Ibid. II. 140.

some judgment of its nature; for, whether the densities depend on different specific gravities of different fluids, or on unequal temperatures of different portions of the same fluid, the curves will be nearly alike.

In each of these cases, to whatever small distance  $p c$ , Fig. 2, the mutual attraction is sufficient to occasion intimate union of the fluids, the density  $m n$  of the mixture will be an arithmetic mean; and, for the same reason, at any intermediate smaller distances, there will be a series of arithmetic means  $e f$ ,  $g h$ , &c. interposed, and the line  $a o$ , uniting the ordinates, will be straight.

By progressive effect of this attraction, and successive interpolations, in Fig. 1, curves  $d e f m$ ,  $r s t m$ , will be formed, of which the straight lines  $m p$ ,  $t q$ , &c. are tangent.

The attracting densities  $n p$ ,  $o q$ , &c. are subtangents; and if it be admitted that these are every where equal, the curves so produced are logarithmic, and the increment of the ordinate greatest at  $m$ , where they meet.

*Prop. 3.* If parallel rays pass through a medium varying according to the preceding proposition, those above the point of contrary flexure will be made to diverge, and those below the same point will converge, after their passage through it.

For since the deviation of each ray depends on the increment of density where it passes, and since the increment of density is greatest at the point of contrary flexure, any rays, as  $a b$ , Fig. 3, passing near to that point, will be refracted more towards the denser medium than those at  $c d$ , which move in a higher stratum, and will diverge from them, but will be refracted towards and meet those at  $e f$ , which pass nearer to the denser medium, where the increments of density are also less.

*Cor.* Hence adjacent portions of the converging rays will form a focus, beyond which they will diverge again; and the varied medium will produce effects similar to those caused by a medium of uniform density\* having a surface similar to the curve of densities, since convergence or divergence will be produced, according as the curve of densities is convex or concave; consequently, by tracing backwards, to the extremities of an object, the progress of the visual rays (or axes of the pencils received by the eye) it will be manifest that,

Any object seen through the inclined concave part  $r m$ , Fig. 5, would appear elevated, erect, and somewhat diminished.

An object seen through  $m d$ , where it is convex and inclined, would be elevated; and, if situated beyond the focus of visual rays from the eye, it would appear inverted. The magnitude would depend on the relative distances of the eye and object.

\* In the varied medium,  $b c$  and  $b m$ , Fig. 4, the corresponding increments of the abscissa and ordinate, are to each other as radius to the tangent of the angle  $c$ . Therefore, the tangent of deviation, which is as the increment of the ordinate, varies as the tangent of the angle  $c$ .

So also, in the uniform medium, since the sines of refraction and incidence are in a given ratio, then differences will bear a given ratio to either of them; and, when the angles are small, the tangent of deviation will vary as the tangent of incidence, or as the tangent of the angle  $c$ , which is equal to it.



Below the point *d*, where the curve terminates, vision would be direct, so that an object might be situated so as to be seen in all three ways at the same time, direct at *O*, inverted at *I*, and erect again at *A*.

I consider the foregoing proposition as applicable to all cases of varying density, whether occasioned by mutual solution of different fluids, or partial rarefaction of the same fluid; and, by trial of various fluids, however different in density, or even in viscosity, I find that the refractions observe a law agreeable to the theory, as will appear by the following experiments:

*Experiment 1.* Into a square phial containing a small quantity of clear syrup, I put about an equal quantity of water, in such a way that it floated on the surface of the syrup without mixing. For a short time, the stratum of union was so thin that nothing could be distinctly seen through it. But soon, by mutual penetration of the water and the syrup, the effects represented at *A*, Fig. 6, were produced.

Through the syrup, a word written on a card, placed behind, was seen erect, and in its place; through the adjacent variable medium, an inverted image was visible above the true place; and also, above that, a second image of the same object appeared erect.

When these appearances are first discernible, the variations of density are so great, that the object to be looked at may be in contact with the phial; but when the variations of density become more gradual, and thereby the focus more distant, any object so near is only elongated, and require to be removed an inch or two, to be seen inverted.

*Exper. 2.* Over the surface of the water, in the same phial, I next put about the same measure of rectified spirit of wine.

At the stratum where the water and spirit united, the appearances were the same; but since the refractive power of spirit exceeds that of water, the true place of the object was seen uppermost; the inverted and erect images are below. Fig. 6, B.

When an oblique line *d e r* is viewed through any variable medium so made, it appears bent into different forms, according to its situation with respect to focal distance.

If it be at the distance of the principal focus, one point of it is dilated into a vertical line, as *l m*. Fig. 7, A.

If beyond that focus the portion *l m* is inclined backwards, being an inverted image of *d l*; and *m n* is another image of the same portion seen erect. Fig. 7, B.

On this account, it becomes a convenient object for ascertaining the state of any medium under examination.

In these experiments, the appearances continue many hours, even with spirits of wine; with syrup, two or three days; with acid of vitriol, four or five; with a solution of gum arabic, much longer; but although their disposition to unite is so different, the appearances produced are nevertheless the same in all.

The refraction is greatest nearly in the plain of original contact of the fluids, diminishing from thence both upwards and downwards. The exact rate of diminution above or below

below this point I had no means of measuring with the accuracy that would be requisite for determining the nature of the curves of density formed according to the first proposition.

But the truth of the second proposition appeared capable of confirmation by experiment; for the deviation of a ray is there said to depend on the increment of density, and time of the ray's passage, jointly; accordingly, the deviation caused by a given increment should be in proportion to the extent of the medium.

In order to try what effect a greater extent of medium would produce,

*Exper. 3.* I made a rectangular glass vessel, of which the sides were in the ratio of 10 to 30,6; and having put into it some clear syrup, with water on its surface, I measured the greatest refractions through it in both directions, and found them in the ratio of 10 to about 29.

In another vessel, of which the sides were as 10 to 40,4, the refractions were, on an average, as 1 to 4.

Being now fully satisfied of the effect of different fluids, I made the following experiment, whereby it appears, that the variations of density occasioned by difference of temperature between adjacent strata of the same fluid, follow a similar law.

*Exper. 4.* Having put some cold water into a square vessel, I covered its surface with a piece of writing paper perforated with a few small holes, and then filled the vessel cautiously with boiling water. The paper nearly prevented any mixture of the hot and cold water; but, by floating gradually up, left them to communicate their heat by contact alone.

While they were in this state, I examined the appearance of remote objects through the varied medium, and found, that when my eye was removed four or five feet from the vessel, the effects were the same as in the preceding experiments with different fluids; above any object viewed through the cold water, I could distinguish two images of it, the one inverted, the other erect, as usual; but these appearances did not continue more than five or six minutes.

Having thus established, by experiments sufficiently varied, that the contiguity of two fluids of unequal density is capable of occasioning all the appearances that have been observed, I shall proceed to shew by what means the air may be made to exhibit similar phenomena.

*Exper. 5.* I heated a common poker red hot, and held it so as to look along the side of it at a paper 10 or 12 feet distant. The rarefaction occasioned by it caused a perceptible refraction to the distance of about  $\frac{3}{8}$  of an inch from the side of the poker. A letter seen more distant from it appeared as usual; within that distance there was a faint image of it reversed; and still nearer to the poker was a second image direct, and as distinct as the object itself, but somewhat smaller, as in Fig 8, in which a section of the atmosphere surrounding the poker is represented. At the bottom and sides it is nearly circular; but upwards the circular form is lost in undulations, occasioned by the rapid ascent of the rarefied air.

The



The greatest deviation produced in this case measured about half a degree.

*Exper. 6.* By a red hot bar of iron, 30 inches long, the refractions were much greater, the extreme deviations amounting to full  $1\frac{1}{4}$  degree.

The refractions observed in these experiments may at first view be thought greater than could be caused by difference of temperature alone, being in one instance more than double the greatest horizontal refraction of the heavenly bodies; in which case, as the rays enter from a vacuum, the greatest possible effect of the atmosphere might be expected. But it must be remembered, that when a star appears in the horizon, its rays intersect the superior strata of the atmosphere at an inclination of several degrees, and that they pass but once through the variations from rarity to density; but, on the contrary, that in the experiments with red hot iron, the rays may pass actually in the direction of the strata, and that they are refracted not only by their entrance from the denser into the rarer medium, but the effect is doubled, since the refraction caused by their emergence is equal to that produced by their incidence.

Although a stratum of air, heated by these means to so great a degree, affords an erect, as well as an inverted, image of objects seen through it, the more moderate warmth communicated to it from bodies heated by the action of the sun upon them, seems insufficient to produce both images; but the inverted image may generally be seen when the sun shines upon a brick wall, or other darker coloured surface.

While the eye of the observer is placed nearly in a line with the wall, if another person, at 30 or 40 yards distance, extends any object towards the wall, an image similar to it will appear to come out to meet it.

It would be difficult to ascertain with accuracy the degree of rarefaction capable of shewing this appearance, but it may be of some use to future observers to mention the different degrees of heat which I observed.

In one instance, a thermometer, in contact with the wall, stood at  $96^{\circ}$ ; but, at  $\frac{1}{8}$  of an inch distance,  $82^{\circ}$ .

One morning, when the sun shone bright, I examined the temperatures and refraction produced at the surface of a deal bar painted green, about eight feet long.

A small thermometer, in contact with the bar, rose to  $96^{\circ}$ ; at  $\frac{1}{4}$  of an inch distance, it stood at  $73^{\circ}$ .

The refraction at the same time exceeded 20 minutes.

To explain why red hot iron occasions two images, while solar heat produces but one, I imagine that the intense heat in the former case rarefies the air for some small distance uniformly, and thereby affords the same series of variations as between other fluids of uniform density; but that, in the latter, the heat is conveyed off as fast as it is generated; so that, as there is no extent of medium uniformly rare, the densities corresponding to the concave portion *r m*, Fig. 5, of the curve before described do not take place, but the phenomena occasioned by the convex part *m d* are alone produced.

It must be remarked, that the vertical position of the surface contributed greatly to encrease the effect; for, since the heated air rises in the direction of the surface, its ascent has in this case no tendency to blend it with the adjacent denser strata, and hence very different degrees of density take place in the thickness of  $\frac{1}{4}$  of an inch; so that, as the increments of density are great, the refractions will be proportionally so; but where the heated surface is horizontal, the ascent of the rarefied air into the superincumbent denser strata renders the variations far more gradual; consequently, a heated surface of far greater extent must be requisite to produce equal refraction.

However, over extensive plains, when the sun shines, some degree of inversion is very frequently to be seen; but the inverted images are rarely well defined, unless over a very even surface. One of the best situations for this purpose is over a level open road, with a gentle breeze blowing across it. A current of air brings a cool stratum more closely in contact with the heated surface; and, since refraction depends on the increment or difference of density in a given small space, a very moderate breeze will thereby render inversion more perceptible; but a strong wind will reduce the temperature of the surface, and may make the heated stratum too thin for any object to be seen through it from a distance.

In one instance, when I saw a refraction of about 9 minutes, at the distance of about  $\frac{1}{3}$  of a mile, a thermometer in the sand was  $101^{\circ}$ ; at 4 inches above,  $82^{\circ}$ ; and, at 1 foot above,  $76^{\circ}$ .

Over water, the evenness of the surface is favourable to the production of such appearances; but, since the action of the sun is weak on a body so transparent, a far greater extent of surface is requisite to produce any perceptible inversion.

Being at Bognor one bright morning, when the sea was calm, I had an opportunity of observing the appearance of Selsea Bill, about six miles distant. The whole extent of coast, when viewed with a pocket telescope magnifying about sixteen times, appeared inverted from one end to the other; and the lower part of a brick house upon the shore was seen as distinct as the house itself. I judged the quantity of refraction, in this case, to be about two minutes of a degree.

This state of atmosphere appears to be not very uncommon; for, at Shanklin Chine, in the Isle of Wight, a few days preceding, similar appearances were visible in several directions, but I neglected to make any estimate of the quantity of refraction.

In the instance of the inverted vessel seen by Mr. Huddart, (*Phil. Trans.* for 1797, Tab. 1, Fig. 3.) at the distance of eight miles the refraction seems to have been about  $3'$ .

All the appearances described by him, I am inclined to think, arose from difference of temperature alone. He offers a conjecture, that evaporation might occasion the lower strata of the atmosphere to have a weaker refractive power; but from the following experiments it seems to have a contrary effect:

*Exper. 7.* I took a plate of glass, and, while I looked along the surface, I poured upon it a small quantity of ether.

A line



A line upon the opposite wall appeared instantaneously elevated many minutes, and at times above half a degree.

This fluid being the most volatile, and most soluble in the atmosphere, of any known liquid, produces the greatest effect; since the cold, during evaporation, conspires with the ether dissolved in the air to increase the refractive power.

Rectified spirit of wine also produces, from the same cause, a very considerable effect.

*Exper. 8.* By moistening a board, five feet in length, with alcohol, and observing the elevation of an object viewed over its surface, I found the refraction to be 15'.

*Exper. 9.* I next made a similar experiment with water itself. Of this, the effect was barely visible, when tried in the same way; but, by means of a surface of ten feet, and by viewing a luminous point at a greater distance, the refraction became evident, and the object elevated above three minutes.

In the course of these experiments, I tried whether confining the saturated atmosphere, by boards on each side, would vary the effect, and found the refraction in all cases much lessened; and when water was used, it became imperceptible; but as soon as the boards were removed, and a free current allowed to pass across, the full effect was again produced. The reason of this difference appears to be, that the quicker evaporation increases the degree of cold, and the current brings greater differences of density contiguous.

The state of rapid evaporation will fully account for the phenomenon witnessed by Mr. Latham, who has described (in the *Phil. Trans.* for 1796, p. 357; or this *Journal*, II. 417) an extraordinary elevation of the opposite coast of France, so as to be seen from the beach at Hastings, and other parts of Sussex.

There is a fact of the same kind stated by De la Lande (*Astron.* Tom. II.) who says that the mountains of Corsica (though at the distance of more than 100 miles) are occasionally visible from Genoa.

It is probably owing to the same cause, that other objects have been sometimes seen, at such distances that we should expect them to be intercepted by the curvature of the earth; for it is evident, that whensoever the evaporation over each mile of surface occasions a refraction of about one minute, the rays receive a curvature equal to that of the ocean, so that its surface will appear flat; and the spherical form of the earth will not obstruct horizontal vision of objects at any distance.

It still remained to explain the phenomena seen by Mr. Vince, as I had not hitherto made an atmosphere capable of exhibiting images inverted, as well as elevated, by increased density. For, in the refractions produced in the 7th, 8th, and 9th experiments, by evaporation at an exposed surface, I observed the effect was always greatest in contact with the evaporating surface; any lower point *a*, Fig. 9, appeared brought nearer to a higher point *c*, by the pencil of rays from *a* being more refracted at *b*, than the pencil from *c* was refracted at *d*. Therefore, any rays passing from the eye

at *e*, as a point, through *b* and *d*, would be made to diverge to *a* and *c*; consequently, visual rays could not, under these circumstances, intersect each other, and no objects could appear inverted.

But whenever the lowest strata of the air become saturated with moisture, the variations between the saturated stratum, and the incumbent atmosphere of the common density, will follow a law similar to what is found at the confines of other fluids of unequal density; hence, inversion will become visible, as there will be a point below which the increment of density will decrease, and where the refractions will consequently be less, although through a denser medium.

*Exper. 10.* To produce these appearances, I procured a trough of thin deal, five feet long, one inch wide, with sides  $2\frac{1}{2}$  inches high, and closed the extremities of it with glass. A section of it is given in Fig. 10.

When the bottom was wetted with ether, the greatest refraction was, at intervals; more than  $\frac{3}{4}$  of an inch from the bottom of the trough; and, beneath this height, I saw a second image inverted, when my eye was removed to 14 or 15 feet distance, and the object was about 70 feet. The focus seemed at the same time to be about nine feet distant.

There was not depth enough of uniformly saturated atmosphere for the object itself to be seen at through it, but its true place, compared with that of the images, is represented at *a*.

*Exper. 11.* When I made use of rectified spirit in the same apparatus, I had also sufficient proof that the laws of evaporation would admit of such appearances being produced; for the same object now appeared curved downwards, as in Fig. 11, so that rays nearer to the bottom were manifestly less refracted than such as passed at some distance above. A degree of convergency must therefore have been produced, although the distance at which the rays would meet was beyond that of my eye, and circumstances would not admit of my removing beyond 35 feet.

The evaporation of water could not be expected to produce any sensible effect of this kind in so short a space; but in a view of some miles extent, there can be no doubt, from the foregoing experiments, that evaporation from the surface of the sea, in such a state of the atmosphere as would allow the lower strata to be saturated, is capable of occasioning all the phenomena which have been described, and probably was the cause of those which Mr. Vince observed.

Since heat alone tends to depress objects, and evaporation produces apparent elevation, it is probable, that in the instance of refraction related by Mr. Dalby (*Phil. Trans.* for 1795, p. 587) the heat of the sun was the principal agent, and that the moisture rather tended to counteract than assist its action.

Simple inversion may generally be seen when the sun shines upon a *dry* even road of  $\frac{1}{3}$  or  $\frac{1}{2}$  mile extent; but when the ground has been wet, I have very rarely seen it, and have even failed of discerning it, when the heat has been sufficient to raise a steam from the ground.



The following experiment shews that it is not to be expected but by very great extent of surface.

*Exper. 12.* I placed a dark coloured board in the sun shine, and having examined the refraction along its surface, I made a wet line along it, with a sponge dipped in boiling water. Notwithstanding this additional heat, the refraction, in the direction of the wet line, was far less than over the rest of the board, although I took care to observe the effect before the surface could be cooled again by evaporation.

I should therefore expect the depression of the horizon at sea where the refraction occasioned by heat must always be counteracted by evaporation, never to exceed a few minutes; and that any one in a situation commanding a view of the sea, by attention to the various degrees of the dip of the horizon under different circumstances, might soon form some estimate of the proper allowance to be made for brightness of the sun at the time of an astronomical observation, or for difference of temperature between the sea and air.

Having now examined the several peculiarities of refraction which I proposed for consideration, I shall, in few words, recapitulate the purport of the foregoing pages.

According to the theory here given, there appear to be two opposite states of the atmosphere, either of which may occasion objects to be seen doubled or tripled, since both increase and decrease of its density, when partial, produce the same effects.

It has been explained,

1st. Why air heated by the moderate warmth of the sun's rays occasions objects to appear doubled and inverted.

2dly. Why rarefaction, by a higher degree of heat, gives an additional image which is not inverted.

3dly. In what state of evaporation the increase of the air's density brings distant objects into view by unusual elevation.

4thly. Under what circumstances evaporation may also produce an inverted image less elevated.

And it is probable that the same reasoning will afford a ready explanation to other varieties of terrestrial refraction, that may have been, or may hereafter, be observed.

## IV.

*A Memoir on the Nature of the colouring Principle of the Lapis-lazuli. By  
Citizen GUYTON\*.*

THE blue stone known by the name of *lapis-lazuli* has long been an object of chemical research, with a view more particularly of discovering that colouring matter which is the cause of its high price, from the lustre which it gives to works in which it is employed, and on account of the preparation of ultramarine blue, which is so highly esteemed as a pigment.

This colour was at first attributed to copper. The celebrated Margraf demonstrated the error of this opinion, but he found that it consisted merely of siliceous earth, sulphate of lime, lime, and a small quantity of iron. Others, since his time, have supposed that oxide of cobalt formed a part of it, whilst others, like Rinman, imagined the fluoric acid to be one of its ingredients. A more strict examination soon destroyed these conjectures.

The methods of analysis having of late years been carried to a degree of perfection beyond the most sanguine expectations, it was natural to think that those chemists who were the most skilful in this new art, would not neglect the application of them to the solution of this important question. Amongst others, I shall refer to Mr. Klaproth, whose works have so much enriched the chemistry of mineral bodies, and who has paid a particular attention to every kind of fossil of a blue colour.

In 1784, he published some experiments, which demonstrated that what is called *native Prussian blue*, which is found in peat grounds, and is often of a white colour before it is exposed to the air, is indebted for its colour to nothing else than a combination of iron and phosphoric acid †.

At Vorau, in Austria, another mineral remarkable for the same colour was discovered, and was successively taken for smalt, or native blue oxide of cobalt, for another kind of native Prussian blue, and for a blue oxide of copper. It is ascertained by the examination of the celebrated chemist of Berlin, that it contains only siliceous earth, alumine, and iron, and though he found that it resisted the action of the fire in a less degree than the lapis-lazuli, he thinks it might be classed among its varieties if it likewise contained lime ‡.

This last conclusion proves that M. Klaproth had, with his usual accuracy, determined the constituent parts of the true lapis. He, in fact, points them out in the tenth article of

\* Read before the philosophical and mathematical class of the National Institute of France 6 Pluviose, An. 3, and inserted in the *Annales de Chimie*, XXXIV. 54.

† *Chemische Annal.* 1784, page 396.

‡ *Beytrage zur Kenntniss der Mineral Korper*, etc. Tom. I. p. 197.---*Annales de Chimie*, XXI. p. 144. (or this Journal, I. 77.)



his researches into mineral substances, where it is observable that the purest lapis, which is called oriental, is a composition of 46 in the 100 of siliceous earth, 28 of carbonate of lime, 14.5 of alumine, 6.5 of sulphate of lime, 3 of oxide of iron, and 2 of water.

He therefore corrected the analysis of Margraff by adding alumine to it, which the other had not noticed, and which more than fifteen years ago I had demonstrated to be contained in it, by touching a plate of the lapis with sulphuric acid, which, at the end of several hours, left very regular crystals of alum, which are preserved upon it.

But by what principle can oxide of iron be tinged with blue without being combined either with Prussic acid, or the acid of phosphorus? M. Klaproth replies that he cannot tell\*.

The experiments, of which I shall now give an account, appear to me to solve this important question; but before I explain the processes and their results, I must, for greater perspicuity, take notice of some operations which led the way to this conclusion.

In the year 1780, whilst I was examining a well at Montolier, on the road from Dole to Poligny, in search of coal, I found, at the depth of 35 metres, a bank of gypsum, containing zones of a fine and very lively red. I gave a description and an analysis of it in the *Journal de Physique* for the month of December in that year, and I inferred from my trials that it was a sulphate of lime coloured by the oxide of iron.

The late discovery of several new metallic substances, some of which have the property of affording colours of great intensity, led me to think that the fossil of Montolier deserved a re-examination with the view of searching for one of these oxides. For this purpose, at the beginning of the present year, I appropriated a piece to those experiments of the mineral analysis which make a part of my course in the polytechnical school. They have been pursued with no less accuracy than intelligence by M. Deformes, formerly a pupil and operator at the laboratory of the second class.

After having ascertained that this mineral does not contain any carbonate of lime, ten grammes of it, reduced to a powder, were ignited in a crucible, by which treatment the colour became deeper, and changed to a yellow brown. There was a loss in weight of 22.3 per cent.

A. Muriatic acid was repeatedly digested on the 77.7 parts remaining after calcination; they did not become discoloured, and the acid only deprived it of a very minute portion of iron.

B. The residue was then boiled in a solution of carbonate of pot-ash, and a very small part only has yet been decomposed.

C. What remained was mixed with charcoal, and treated in the crucible. A sulphuret being formed, it was decomposed by the muriatic acid, which seized the lime and iron. The siliceous earth remained mixed with the superabundant coal.

\* *Beytrage*, &c. I. 201.

D. The liquids of the three preceding operations were put together, and then divided into two equal portions. From the first the iron was separated, and was found to weigh 10.5, and it was ascertained that it contained no other earth except lime. From the second, eight decigrammes or hundredth parts of lime, were obtained.

The whole of the solution, therefore, contained :

Oxide of iron	-	-	21.
Lime	-	-	16.
Sulphuric acid	-	-	29.1

the last according to the proportions determined by Klaproth.

E. It remained to examine the portion of earth left by the acid, and which was mixed with the superfluous coal. It is here that the operations began to present unexpected phenomena, and which, by deviating from the usual course, advise the attentive chemist that he is on the eve of discovery.

This residue was at first calcined in the open air, in order to burn the coal, but the siliceous earth remained black : its weight was 16.5.

It was treated with pot-ash in a crucible of platina, and yielded a fusible mass of a superb blue colour.

Water poured upon it assumed the same colour.

Nitric acid made it totally disappear.

The siliceous earth separated by evaporation to dryness weighed only 8.6.

F. It became of consequence to know the effect of the different re-agents on the acid which had been applied to the mass fused in the crucible, which might discover the substance which afforded its properties. The following is the result of these trials :

1. With prussiate of pot-ash this fluid yielded a precipitate of a yellowish green colour, which the addition of acids caused instantly to disappear instead of reviving the blue.

2. With the gallic acid there was no precipitate.

3. With sulphurated hydrogen there was no precipitate. This, indeed, was to be expected upon the supposition that iron alone was present, but it was proper in this manner to exclude the other metallic substances which are precipitated by this re-agent.

4. With hydro-sulphuret of ammoniac there was a fine green precipitate.

5. With ammoniac there was a white precipitate.

6. With pot-ash there was a light blue precipitate.

The two last colours changed, in the drying, to a yellow.

Some comparative experiments were made at the same time with a solution of nitrate of iron, and the result of them was totally different.

G. That there might remain no doubt respecting the nature of the substances by whose presence the results of these operations was affected ; synthesis was called in to the assistance of analysis. Sulphuret of iron was prepared in the direct way ; a sufficient quantity of nitrous acid was poured upon it ; the filtered liquid was diluted with much water, in order that the excess of acid might no longer precipitate the sulphurated hydrogen which



which was poured upon it. In this state the same re-agents, which had been employed in the former experiments presented the same phenomena. It might be imagined that sulphate of unoxidized iron would, in the same circumstances, afford similar effects; but prussiate of iron afforded only a white precipitate, as had been announced by Professor Proust.

H. It was now easy to draw a conclusion respecting the analysis of the red sulphate of lime of Montolier. It confirmed the observation I had made that it contained nothing but oxide of iron, and determined besides the quantities of its constituent parts, which are as follows :

Sulphuric acid	-	-	-	29.1
Lime	-	-	-	16.
Oxide of iron	-	-	-	21.9
Siliceous earth	-	-	-	8.6
Water driven off by the first calcination	-	-	-	22.3
Loss	-	-	-	2.1
				<hr/> 100.0 <hr/>

The termination, however, of this subject opened a path to new enquiries, of which it is now time to give an account.

I. From observing the blue colour which the sulphate of lime of Montolier had assumed by the addition of pot-ash, the effects of the re-agents upon the acid, in which its iron is held in solution, and more particularly the disappearance of the green precipitate formed by prussiates by the addition of an acid, M. Deformes immediately recollected that he had observed phenomena perfectly similar in some experiments performed during last year with Citizen Clouet upon the lapis-lazuli. This fact already afforded a strong indication that the stone in effect contains no other colouring metallic oxide than iron. It remained therefore only to pursue the coincidence of the facts in all their circumstances, to determine the particular state in which this metal is found when it produces this beautiful blue composition. Some experiments added to the observations already known upon the properties of the lapis-lazuli will form this connection.

The lapis-lazuli may be urged to a red heat, and even lose 0.2 of its weight, without any visible alteration in its colour; but with a stronger heat, such as that of the furnace of an enameller, its colour changes to grey. By still encreasing the intensity of the fire, it is reduced to a brownish glassy scoria, with a diminution of from 10 to 11 hundredths of its weight.

L. When the lapis is pulverized, a smell of musk is sometimes perceived, which alumine and magnesia equally afford when they are united with a little sulphur.

M. The lapis is discoloured with more or less quickness by the three acids which are called mineral. The nitrous acid acts almost instantaneously; the muriatic acid is less speedy; and the sulphuric acid acts the most slowly. These acids often disengage the same smell as is produced by trituration.

If

If the nitrous acid be concentrated, it affords nitrous gas, and sometimes when the lapis contains carbonate of calx, carbonic acid gas. The liquid, when tried by the prussiates, affords a precipitate whose colour resembles that of Prussian blue, but visibly inclining towards a green which is destroyed by acids. The hydro-sulphuret of ammoniac occasions a precipitate inclining to black.

When nitrous acid, diluted with water, is used, there is a disengagement of a small quantity of sulphurated hydrogen. The prussiates then form only a bright green precipitate in the liquid, which is instantly destroyed by acids: with hydro-sulphuret of ammoniac the precipitate is of a fine green.

When the lapis has been previously subjected to calcination, the dilute nitrous acid disengages a little of the sulphureous acid gas.

N. These facts not only prove that the lapis-lazuli contains a small portion of sulphur, but they also demonstrate the identity of the colouring principle of this stone with every composition in which earthy substances enter into combination with sulphuret of iron. For we have seen (F. G.) the sulphate of lime containing iron, and converted by charcoal to the state of sulphuret of iron; and the sulphuret of iron, prepared in a direct manner, present the same phenomena under the same circumstances.

O. Before I conclude this memoir, I shall make a few observations, to guide those persons who may be disposed to repeat these experiments.

The composition of every specimen of lapis-lazuli is not essentially the same. In many pieces, and even in what is called the oriental, we distinctly perceive the sulphuret of iron in crystals with the metallic brilliancy; sometimes it is disseminated in small portions; and this undoubtedly is the cause which has hitherto prevented the most accurate chemists from ascertaining the true colouring principle of this substance. They have considered the sulphur merely as an accidental product foreign to the subject of analysis, without suspecting that there existed a blue sulphuret of iron. It is obvious that in experiments of this kind the greatest attention should be paid to the choice of fragments absolutely exempt of all pyritous admixture, or sulphuret of iron of a metallic yellow.

The presence of this last sulphuret is not the only difference which is found in the specimens of the lapis. Out of three kinds which were subjected to experiment, one contained sulphate of lime and siliceous earth, with crystalized sulphuret of iron, and the blue sulphuret of the same metal: the second also contained barytes: the third, which was absolutely free from pyritous admixture, contained also alumine and carbonate of lime in its composition, like that which was analysed by M. Klaproth.

The fact of crystals of alum being rapidly formed upon a fine plate of the lapis, as before remarked, proves beyond all doubt that some specimens contain accidentally a small quantity of pot-ash. It will be proper, therefore, to seek the colouring principle amongst those component parts which are essential to the stone, and not to be diverted too much by these accidental variations. We must not, however, suppose that the affinity of the earths to each other, or to the colouring principle, is without their influence upon the nature of the compound.



compound. A very strong proof to the contrary offered itself in one of the synthetical operations. Sulphate of lime abounding with iron having been treated with powdered charcoal, and then kept in digestion in nitrous acid; the prussiate of pot-ash at first had no other effect but to turn the fluid green, without affording any precipitate; the addition of a solution of alumine immediately determined a green precipitate, which was taken up by acids, and was acted upon in all respects like that which was obtained from the decomposition of the lapis.

### CONCLUSION.

I return to the consequences which appear to me to result from the facts laid down in this memoir.

1°. The sulphate of lime of Montolier is coloured by a red oxide of iron, which adheres so strongly to the siliceous earth as to resist the action of acids.

2°. This sulphate, treated with charcoal, produces a sulphuret of iron, in which this metal is less oxidized, which being dissolved in acids, no longer affords a Prussian blue by the prussiates, but a green precipitate, which is destroyed, instead of being brightened, by acids, and which preserves its peculiar blue colour in pot-ash itself, and in the fire which its dry fusion requires.

3°. In operating upon sulphuret of iron prepared in the direct way, a product is obtained which manifests the same properties in the same acids, and by the same re-agents.

4°. These phenomena are exactly similar to those which the lapis presents when it is subjected to the same operations.

5°. So that we can at pleasure form the blue colouring principle of the lapis, with the only difference which necessarily results in the natural product, from the slow combination of the principle with the earths and the sulphate of lime.

6°. In a word, the blue sulphuret of iron is the true and only colouring principle of all the varieties of the lapis, and probably also of the mineral known by the name of blue stone of Vorau.

### V.

*Experiments made with the Metallic Pile of Signor Volta, principally directed to ascertain the Powers of different Metallic Bodies. By Lieut. Col. HENRY HALDANE.*

#### LETTER III.

TO MR. NICHOLSON.

S I R,

THE apparatus I have used in the following experiments is of a very simple construction. A deal board, of about six feet long, three inches wide, and one inch in thickness. The middle of the board is fluted longitudinally, the channel being 0.6 inch wide. The

metallic pieces, which were used, being circular, and about 1.3 inch diameter, rested upon the edges of this groove, and which, with a small block of wood at each extremity or base, served to keep them in their places. The board being inclined to the horizon, the channel carried off the water that oozed from the wetted discs.

In those experiments in which mercury was combined with other metals, it became necessary, on account of the fluidity of the mercury, to adopt some other contrivance for the apparatus, by which the flowing of the mercury might be prevented. To effect this, I procured 20 flat pieces of mahogany of two inches square, and of 0.3 inch in thickness: in the middle of each of these pieces was made a circular hole, of one inch diameter on the lower side, and of 1.5 inch diameter on the upper side, forming a rabbet at about two-thirds of the thickness of the wood from the upper surface. The lower side was then covered with a piece of leather attached to it with a cement, which was not easily acted upon by the water used in the experiment; and the leather was turned up the sides of the wood to make it more secure. The cement I used is made of melted wax, rosin, and red ochre.

When this apparatus was to be used, the pieces of wood were put into pure water for a sufficient time to soak the leather; and when taken from the water, the mercury was poured into the small cisterns, filling them to the edge of the rabbet; the solid metal was then laid upon the rabbet, having its lower surface in contact with the mercury. If this solid metal was not of sufficient thickness to be level with the surface of the wood, a wetted disc was laid upon it to fill up the vacant space. These small blocks of wood, thus prepared, being piled upon each other, the metallic substances they contained became capable of acting as a galvanic apparatus.

The order in which these experiments are arranged is according to the degrees of influence, the different metals seemed to possess, in forming the oxydating bases or poles of the apparatus, when any oxydable metallic wires, placed in tubes of water, were submitted to examination; the other metal constituting the opposite or gaseous base or pole. The discs, that were used, were sometimes of card, and sometimes of leather, and were soaked in pure water. Though the epidermis of an animal is obviously an insulating substance, with respect to the galvanic influence (but loses a part of that property by being wetted) yet I have not discovered any difference in the effect, whether wetted card, or wetted leather, was employed; the leather has the advantage of retaining the moisture, which is so absolutely requisite in the whole operation of a galvanic apparatus, a longer time than the card; but it discolours very much the adjoining surfaces of the metallic plates. It seems to be only necessary that each pair of plates should be separated from the adjacent pairs by some substance that is not a perfect conductor of the galvanic influence, a property which seems to be confined to metalline substances; and that the separating substance or disc should be constantly wet. The wire used in these experiments was of copper, and the glass tubes were filled with pure water.



*Experiment 1.*—Zinc, combined with gold—silver—iron—copper—lead—tin—mercury.

The results of all these combinations were:—the caustic sensation felt by the tongue; the irritation and galvanic shock; and the oxydation of the copper wire. The zinc always forming the oxydating base or pole, and the other metals constituting the gaseous base or pole.

In the combination with gold, (which was 40 guineas, and the zinc, the dimensions of an half crown piece) the galvanic apparatus seemed to act with more vigour than under any other combination, and which was made apparent by comparing it with an apparatus composed of shillings instead of the half crowns, which were employed in all the other combinations in which silver was used. In the combination of zinc with silver, the apparatus, which was composed of 40 pairs of zinc and half crowns, acted with about the same power as the gold apparatus; but this difference might be observed, that in the gold apparatus the wire connected with the zinc base or oxydating pole began, after having deposited much green oxide of copper, to give out bubbles of air, when, in the silver and other apparatus, the wire connected with the gaseous base, after a short time, began to deposit oxide; that is, in the gold apparatus the power of producing gas seemed to predominate, and in the other combinations with zinc, that of producing oxide.

The combinations of zinc with the other metals produced much oxide and gas, and their powers of acting seemed to be in the order of iron, copper, lead, tin, mercury. In the combination with mercury, the effects were the most feeble. The surface of the zinc in contact with the mercury became, of course, much amalgamated.

The green oxide of copper in the tubes was examined by putting some drops of ammoniac (*aqua ammonia pura*) into each of the tubes; the green oxide was dissolved, and the water acquired a blue tinge, but some flakes of a brown substance remained in the tubes, not acted upon by the ammoniac. This brown matter was not always formed by all combinations, but the silver and copper always produced it.

*Experiment 2.*—Iron, combined with gold—silver—copper—lead—tin—mercury.

In these combinations the results were nearly similar to those of the first experiment, the iron forming the oxydating base or pole, and the other metals constituting the opposite or gaseous base or pole; but these apparatus acted more feebly, than the combinations with zinc, excepting when iron was combined with mercury, with which it seemed to act as powerfully as any combination in the first experiment. With gold and silver, iron seemed to act tolerably well, produced much oxide, and gas. With copper it acted more feebly, and very minute bubbles only of air were produced. With zinc and tin, it only formed a cloud in the water, and no gas appeared at the extremity of the opposite wire. The powers of these combinations with iron appeared to be in the order of mercury—gold—silver—copper—lead—tin.

The different colours and quantity of rust formed upon the surfaces of the iron pieces when combined with mercury, gold, and silver, compared with what was formed in

the other combinations of iron, were worthy of notice. With gold, the rust was very red and very bright. The part of the iron in contact with the mercury retained its metallic lustre.

*Experiment 3.*—Lead, combined with gold—silver—copper—tin—mercury.

In these combinations the operations of the galvanic apparatus were very feeble. The lead formed the oxydating base. When combined with gold or silver, oxide was deposited from the wire connected with the lead, and the other base produced some gas. With copper, the oxide was more sparingly formed, and very minute bubbles of air appeared at the extremity of the wire connected with the copper. With tin, only a small cloud appeared at the extremity of the wire connected with the lead, and no gas was formed by the opposite wire. With mercury, 20 pieces of lead acted very feebly; scarce any effect was perceptible, a small speck of a cloud was visible at the extremity of the wire connected with the lead, and no gas at the opposite wire: but from the causticity which at first was perceptible, it appeared that these metals in greater quantities might be capable of acting. The lead was, of course, much amalgamated by the mercury.

*Experiment 4.*—Tin, combined with gold—silver—copper—mercury.

These combinations acted more feebly than any in the former experiments. The tin, when any effect was apparent, was always the oxydating base, but it produced only a cloud in the water in the glass tube. With 44 pieces of gold (guineas) a small quantity of gas appeared, but with silver, or copper, no gas was visible at the extremities of the wires connected with those bases, till the apparatus was increased to 160 pairs of metallic plates. With mercury, no effect was produced, not even causticity was perceptible, and the tin, which was 20 circular pieces of tin-foil, was soon amalgamated, and nearly dissolved by the mercury.

*Experiment 5.*—Copper, combined with gold—silver—mercury.

In these combinations, no effect was visible, excepting in the combination of 67 pairs of copper and silver. The wire which was connected with the copper, was surrounded with small bubbles of air, and formed a cloud in the water, but the wire connected with the silver base, produced no gas. With 56 guineas the copper (penny pieces that have been always used) produced no oxide and no gas, but when the apparatus was put together, a caustic sensation was perceptible upon touching it with the tongue, and which seemed to indicate a tendency to act as a galvanic apparatus if the number of metallic plates had been increased. With mercury, 20 plates of copper produced no effects whatever.

*Experiment 5.*—Silver, combined with gold.

In this combination of 30 pairs of plates, no causticity, and no effect whatever, was perceptible.

Judging from the results of former experiments, it was not thought necessary to combine either silver or gold with mercury, as no effect could be expected that was likely to compensate for the certain amalgamation of those metals; but it would have been very desirable to have extended these enquiries to the effects which the other metallic substances combined



combined in the form of a galvanic apparatus would produce, particularly *platina*; whether it would act with the other metals in a manner similar to gold or silver, forming always the gaseous base of the apparatus; or whether it would be capable of acting with those metals like zinc, iron, &c. and form the oxydating pole of the combinations; but *platina*, particularly in a malleable state, was found, upon enquiry, to be too expensive an article to be employed by an individual for this purpose, especially at a time, when money is so frequently required for many other experiments; indeed it might, in some degree, be submitted to examination in its granular form, by means of the apparatus which I used in the experiments with the quicksilver.

*Experiment 6.* The object of this experiment, was to examine the effects produced by increasing the number of pieces that compose a galvanic apparatus; and to ascertain whether its power increases in a greater proportion, by augmenting the number of metallic plates, or by extending their surface.

The first apparatus consisted of 200 half crown pieces, combined with 200 pieces of zinc of the same diameter; and the discs were leather soaked in pure water. This apparatus was placed upon the board before described, extending in length 3 feet 9 inches. The effects of this combination were very feeble in proportion to its dimensions; when compared with an apparatus of forty pair of plates, its power was not increased in the proportion of 5 to 1. It was then re-constructed, and put together with leathern discs soaked in a solution of muriate of ammonia (*sal ammoniac*).

The result was, that this apparatus acted very powerfully; the causticity, and the galvanic shock were more severe; the oxidation of the copper wire was more rapid; and the bubbles of air more copiously formed in the tubes of water. When the tongue was made part of the circuit, the sensation, which in the smaller apparatus seemed to be rather caustic, resembled in this a pulsation, upon every repetition of the shock; when the shock was received upon the cheek, a flash of light appeared before the eyes, as when the tongue had formed a part of the circuit. When two wires connected with the bases of this apparatus, were immersed in separate vessels of water, and the fingers of each hand were brought into contact with the water only, a galvanic shock was very perceptible, and a continued numbness was felt, if the fingers were permitted to remain in the water. From this experiment it is obvious, that the increased effects of a galvanic apparatus, depend more upon the nature of the substances which enter into its composition, than upon the number of metallic plates.

## PART II. EXPERIMENT 6.

In this experiment, it was proposed to examine the effects of a galvanic apparatus composed of 20 pairs of metallic plates, each six inches diameter, but being disappointed in obtaining the plates of zinc, the experiment has been deferred. The combinations of lead, iron, lead, and tin, have been examined, and from the increased effects which they exhibited, and which are always very feeble, there is little reason to expect, that the increased

creased surfaces of the plates in a galvanic apparatus, will be productive of proportionably increased power.

*Experiment 7.* This experiment is not very dissimilar to that which you have related in your Journal, (page 185) in which you proposed to examine the aeriform matter, separately, that was produced by each of the bases or poles of a galvanic apparatus. In this experiment, an electric explosion was passed through the gases which issued from the two extremities of gold wire connected with the opposite bases of the apparatus.

The apparatus that was used was composed of 200 plates of zinc, and 200 half crown pieces; the discs were of leather soaked in a solution of muriate of ammonia; the extremities of two pieces of gold wire were introduced into a small glass vessel filled with pure water; having an interval between them of  $\frac{1}{4}$  of an inch. This small vessel being completely immersed in a larger vessel of water, was inverted, and the projecting ends of the gold wire attached to the bases of the apparatus.

The result was, that gas issued most copiously from both the extremities of the wire, immersed in the water, and ascended to the upper part of the vessel, where it was collected in a large bubble, in the course of the time the apparatus continued to act, which was about thirty hours. The wire connected with the zinc base was 3 feet 6 inches in length, and the wire connected with the silver base, about one foot. The former became much tarnished, gave out much air, but not so rapidly, or for so long a time, as the opposite wire. The part of the wire connected with the silver base, that passed through the water in the outer vessel, also gave out much air, and became at last incrustated with some white substance; upon the bubbles which were constantly ascending from this wire, a small quantity of tincture of turnsole was dropped, but it underwent no change of colour. No oxide, or even a cloud, was produced in the water by either of the wires.

The gas thus collected was decanted into a glass tube, which was bent, the branches being about four inches long, and two inches asunder; the interior diameter was 0.3 inch, and the gas occupied 1.5 inch in the curved part of this tube. The branches being then placed in separate vessels of water, and two metallic wires having been previously fixed in this tube, having a small interval between their extremities at the part where the bubble rested; the discharge of a small electrical jar was passed through these wires, and the explosion which took place in the bubble, had sufficient force to raise the tube, the weight of which, with the wire and water it contained, was about three ounces. The bubble of air was reduced to less than half its original dimensions, either by a diminution of its quantity, or of its elasticity. Many electrical explosions were then passed through the remaining air, and also through it, when mixed with atmospheric air, but without producing any motion in the tube, or any other effect.

The tube was then removed, and a small quantity of a mixture of oxygenous, and hydrogenous gas, which happened to be ready prepared, was admitted into it, so as to form a bubble of the same dimensions as before; and upon passing the discharge of the electrical jar through it, the effects were precisely the same, as were observed in the air extricated from the gold wire, by the operation of the galvanic apparatus.

*Experiment*



*Experiment 8.* By the 7th experiment contained in my letter of the 24th of June, it appears that a galvanic apparatus is not capable of acting in a *Boylean vacuum* \*. In this experiment, I introduced into three separate glass receivers (of 1 foot 3 inches in height, and 5.5 inches diameter) placed over water, three galvanic apparatus, each of forty half crowns with zinc, and the discs soaked in pure water. The first receiver was full of atmospheric air; the second had been filled with oxygenous gas prepared from manganese and sulphuric acid; and the third contained azotic or nitrogene gas, which had been obtained by leaving a mixture of 1.5 ounces of iron filings, with sulphur in it for five days, during which time the water had risen in it about four inches. Three glass tubes filled with pure water as before, and with copper wires, were joined to brass wires, which had been previously fixed to the bases of the apparatus, and passing under the bottom of the receiver, were extended above the surface of the water in the cisterns.

The result of this experiment was, that the copper wires attached to the apparatus in the receiver filled with atmospheric air, produced oxide and gas as usual, but not in such quantities as when the apparatus has free access with the external air. The wires attached to the apparatus in the receiver of oxygenous gas, formed gas and oxide most copiously, even part of the brass wire attached to the zinc base, deposited much oxide within the receiver. The wires attached to the receiver of azotic gas, produced neither oxide or gas, and the galvanic apparatus had no apparent effect.

The tubes after remaining twenty hours were removed, and new wire attached to the apparatus in the receivers containing atmospheric air, and the oxygenous gas; but they produced only a faint cloud in the water. The water had risen in the receiver, which manifestly indicated a diminution of the original quantity of air.

#### CONCLUSION.

From this experiment, and from the examination of the state to which atmospheric air is reduced in a glass receiver, placed over a galvanic apparatus and confined by water, I think we may venture to agree in opinion with Cit. Fabroni, (*Phil. Journal*, vol. III. p. 308) that the effects of galvanism depend on a chemical operation, and are produced principally by the attraction of oxygen from the atmosphere, and therefore, on the present theory, the whole operation can be received only as a combustion similar to that which arises from the combination of sulphur, and iron filings with water.

I remain, Sir,

with much esteem,

your most obedient,

humble Servant,

Croydon, Aug. 3, 1800.

HENRY HALDANE.

I have submitted to examination sulphur and iron filings, in the apparatus I used for the quicksilver; but I do not perceive that that combination produces any effect as a galvanic apparatus.

\* Vide Mr. Boyle's Letter to Lord Dungarvon, page 10, Quarto.

## VI.

*Investigation of the Powers of the Prismatic Colours to heat and illuminate Objects; with Remarks that prove the different Refrangibility of radiant Heat. To which are added, an Inquiry into the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers, and Experiments on the Refrangibility of the invisible Rays of the Sun. By WILLIAM HERSCHEL, L. L. D. F. R. S\*.*

THIS eminent philosopher begins his first paper by observing, that it is sometimes of great use in natural philosophy, to doubt of things which are commonly taken for granted; especially as the means of resolving any doubt when once it is entertained, are often within our reach. Whence it may be affirmed, that any experiment which leads us to investigate what was before admitted upon trust, may become of great utility to natural knowledge. Thus for instance, when we see the effect of the condensation of the sun's rays in the focus of a burning lens, it seems to be natural to suppose, that every one of the united rays contributes its proportional share to the intensity of the heat which is produced; and we should probably think it highly absurd, if it were asserted, that many of them had but little concern in the combustion or vitrification which follows when an object is put into that focus. From these considerations, he has thought fit to mention what led him to surmise, that the power of heating and illuminating objects might not be equally distributed among the variously coloured rays.

In a variety of experiments occasionally made, relating to the method of viewing the sun with large telescopes to the best advantage, he used various combinations of differently coloured darkening glasses. What appeared remarkable was, that when he used some of them he felt a sensation of heat, though he had but little light; while others gave much light, with scarcely any sensation of heat. Now as in these different combinations the sun's image was also differently coloured, it occurred to him that the prismatic rays might have the power of heating bodies very unequally distributed among them; and as he judged it right in this respect to entertain a doubt, it appeared equally proper to admit the same with regard to light. If certain colours should be more apt to occasion heat, others might, on the contrary, be more fit for vision by possessing a superior illuminating power; and at all events, it would be proper to recur to experiments for a decision.

In the first series of experiments *on the heating power of the coloured rays of the sun*, a piece of pasteboard was mounted in a frame, so that its obliquity could be varied with respect to the horizon, nearly in the same manner as in the common table looking glass. Through this pasteboard was cut a notch, or slit, of width a little larger than the ball of a thermo-

\* Abridged from two papers in the Philosophical Transactions for 1800, pages 255---292. The titles of both are united in the above, with no other variation than the insertion of the conjunction *and* before the word *experiments*, which begins the second title; and the necessary change of the word *is* for *are*.---N.



meter, and of sufficient length to permit the whole of one of the prismatic colours to pass through; so that in the actual exhibition, this slit was placed parallel to the axis of a prism through which the solar rays pass, and the surface of the pasteboard was adjusted at right angles to the ray itself. Three delicate thermometers with their balls blacked, and sufficiently detached from their respective scales, were placed on the platform of the frame, at such a distance beneath the opening in the pasteboard, that any one, or all of them, might at pleasure be exposed to the coloured rays, or shaded from the light by the interposition of the unperforated surface. When one of these was exposed to the rays, its mercury rose, while that in the other two remaining stationary, proved, that the accession of heat was produced by the mere action of the solar light. Eight sets of experiments were tabulated, from which it is seen, that the mean rise in one of the thermometers in red rays, during ten minutes was  $6\frac{1}{2}$  degrees; and in the green rays  $3\frac{1}{4}$  degrees; and in the violet rays two degrees. With a smaller thermometer the same effect followed, but the alteration was less, probably from the cooling agency of the ascending stream of air, which would act more strongly where the proportion of surface to bulk was greater. The mean results from both thermometers were, that the degrees of elevation in the red, green, and violet rays, proved nearly as the numbers  $3\frac{1}{2}$ ,  $1\frac{1}{2}$ , and 1 respectively.

The second course of experiments described in this paper, was made *on the illuminating power of coloured rays*; in which the author had two ends in view, the first with regard to the illumination itself, and the second with respect to the aptness of the rays for giving distinct vision: properties which, though there did not seem to be any reason why they should not have the same measure, appeared nevertheless to deserve to be separately attended to.

The microscope offered itself as the most convenient instrument for this investigation; and this was used as upon opaque objects, in order to avoid any effect that might be expected to arise from transmission through the parts of coloured transparent bodies.

The register of a number of experiments, in which different objects were viewed in the coloured rays by a magnifying power of 42 times, is given; among these a nail is mentioned as peculiarly suited for researches of the present kind. It was chosen on account of its solidity and blackness, as being most likely to give an impartial result of the modifications arising from an illumination by differently coloured rays; but on viewing it, Dr. H. was struck with the sight of a bright constellation of thousands of luminous points, scattered over its whole extent, as far as the field of the microscope could take it in. Their light was that of the illuminating colour, but differed considerably in brightness: some of the points being dim and faint, while others were luminous and brilliant. The brightest of them also admitted of a little variation in their colour, or rather in the intensity of the same colour; for in the center of some of the most brilliant of these lucid appearances, their light had more vivacity, and seemed to deviate from the illuminating tint towards whiteness, while on and near the circumference it seemed to take a deeper hue.

The observations which agreed uncommonly well together, afforded the conclusion that the red making rays were far from possessing an eminent degree of illumination: that the orange possesses more of it than the red; the yellow still more; the maximum of illumination lies in the brightest yellow, or the palest green; the green itself is nearly as bright as the yellow; but from the full deep green the illuminating power decreases very sensibly. That of the blue is nearly upon a par with the red; the indigo much less than the blue; and the violet is very deficient. With regard to the principle of distinctness, none of the colours appeared to be deficient; that is to say, that though for want of illumination in the less powerful colours in this respect, fewer bright spots could be discerned, yet those which were visible were perfectly distinct.

Before the Dr. proceeds to the next part of his subject, he digresses for a moment to remark, that the foregoing researches ought to lead us on to others. "May not" says he, "the chemical properties of the prismatic colours be as different as those which relate to light and heat? Adequate methods for an investigation of them may easily be found; and we cannot too minutely enter into an analysis of light, which is the most subtle of all the active principles that are concerned in the mechanism of the operations of nature. A better acquaintance with it may enable us to account for various facts that fall under our observation, but which have hitherto remained unexplained. If the power of heating, as we now see, be chiefly lodged in the red-making rays, it accounts for the comfortable warmth that is thrown out from a fire, when it is in the state of a red glow; and for the heat which is given out by charcoal, coke, and balls of small coal mixed up with clay used in hot houses; all which it is well known throw out red light. It also explains why the yellow, green, blue, and purple flames of burning spirits mixed with salt, occasion so little heat, that a hand is not materially injured, when passed through their confluents. If the chemical principles of colours also when ascertained, should be such, that an acid principle, for instance, which has been ascribed to light in general, on account of its changing the complexion of various substances exposed to it, may reside only in one of the colours, while others may prove to be differently invested, it will follow, that bodies may be variously affected by light, according as they imbibe and retain, or transmit and reflect, the different colours of which it is composed."

*That radiant heat is of different refrangibility*, is also deducible from these experiments, whether it be one and the same thing, or different from light; because, as the author observes, if this were not the case, the whole of the heat would be confined in a space equal in breadth to the prism itself; the contrary to which is proved by the facts. He also states, as is more fully developed in the subsequent paper, that as the maximum of light is found at a much higher degree of refrangibility than that of heat in the visible spectrum, the absolute maximum lies even short of the limits of visible radiation in the spectrum. And admitting, as is highly probable, that the organs of sight are only adapted to receive impressions from particles of a certain momentum, an explanation will be had, why the maximum of illumination should be in the middle of the refrangible rays, as those which have

greater



greater or less momenta are likely to become equally unfit for the impressions of sight. Whereas in radiant heat there may be no such limitation to the momentum of its particles. From the powerful effects of a burning lens, however, the information is gathered, that the momentum of terrestrial radiant heat is not likely to exceed that of the sun; and that consequently the refrangibility of calorific rays cannot much extend beyond that of calorific light. Hence also it is inferred, that the invisible heat of red hot iron gradually cooled till it ceases to shine, has the momentum of the invisible rays, which in the solar spectrum, viewed by day light, go to the confines of red; and this will afford an easy solution of the reflection of invisible heat by concave mirrors.

The results of the foregoing investigation, originally suggested by the phenomena of vision by telescopes directed to the sun, became useful in their turn to direct the processes, by which a method might be obtained of *viewing that luminary with telescopes of large apertures and high magnifying powers*. The focal heat in the large telescopes used by the Doctor, was sufficient speedily to break the darkening slips of wedge-formed glasses, commonly used with achromatic telescopes. It was a Newtonian reflector of nine inches aperture, which he wished to adapt for solar inspection, and his aim was to employ the whole surface of the speculum. Two red glasses intercepted full as much light as was necessary; but the eye could not bear the irritation arising from a sense of heat. Green glasses, one of which was smoked, still gave a brighter illumination than the red, but they remedied the inconvenience of the heat. Various trials for intercepting the red, and other rays from a prism, with differently coloured glasses, were made; of which the particulars are given, and also of the effects of a coating of smoke and of pitch, and of coloured fluids, the latter of which when dense enough to stop much light, were not found sufficiently pure to be used.

These last trials, however, were not sufficient to ascertain the very essential particular of distinctness afforded by these several media. It was necessary to try the several glasses and combinations in the actual instrument, previous to the account of which the Doctor describes an easy way of uniformly smoking glasses, which I shall proceed to copy.

“With a pair of warm pliers take hold of the glass, and place it over a candle at a sufficient distance not to contract smoke. When it is heated, but no more than still to permit a finger to touch the edges of it, bring down the glass at the side of the flame as low as the wick will permit, which must not be touched. Then, with a quick vibratory motion, agitate it in the flame from side to side; at the same time advancing, and retiring it gently all the while. By this method you may proceed to lay on smoke to any required darkness. It ought to be viewed from time to time, not only to see whether it be sufficiently dark, but whether any inequality may be perceived; for if that should happen, it will not be proper to go on.

“The smoke of sealing wax is bad: that of pitch is worse. A wax candle gives a good smoke: that of a tallow candle is better. As good as any I have hitherto met with, is the smoke of spermaceti oil. In using a lamp you may also have the advantage of an even flame extended to any length.”

The telescopic experiments, with various darkening glasses applied in the eye piece, are numbered and described. Difficulties presented themselves, from the heat which passed through some combinations, and the want of light and distinctness in others. The heat which was intercepted when the glasses were placed near the focus of the pencils, was also found to break them by its partial action. To remedy this, one of the glasses was fixed near the small speculum, in order that the light might be spread over a larger surface; but here also the heat was too strong, and produced the same inconvenience. The Doctor therefore at last placed his apparatus close behind the eye glasses, as follows in his best combination.

"No. 25. I placed a very dark green glass behind the second eye glass, that it might be sheltered by both glasses, which in my double eye-piece are close together, and of an equal focal length. Here, as the rays are not much concentrated, the coloured glass receives them on a large surface, and stops light and heat in the proportion of the square of its diameter now used, to that on which the rays would have fallen, had it been placed in the focus of pencils. And for the same reason, I now also placed a dark green smoked glass close upon the former, with the smoked side towards the eye, that the smoke might likewise be protected against heat by a passage of the rays through two surfaces of coloured glass."

"This position had moreover the advantage of leaving the telescope, with its mirrors and glasses, completely to perform its operation, before the application of the darkening apparatus; and thus to prevent the injury which must be occasioned, by the interposition of the heterogeneous colouring matter of the glasses and of the smoke."

"No. 26. I placed a deep blue glass, with a blueish green smoked one upon it, as in No. 25, and found the sun of a whiter colour than with the former composition. There was no disagreeable sensation of heat; a little warmth might be felt."

These two are the combinations through which the Doctor has seen uncommonly well, and in a long series of very interesting observations upon the sun, which will soon be communicated, the glasses have met with no accident. However, when the sun has considerable altitude, he finds it advisable to lessen the aperture a little in telescopes, which have so much light as his ten feet reflector, or (which will give more distinctness) to view the sun earlier in the morning, and later in the afternoon; because the light intercepted by the atmosphere in lower altitudes, will reduce its brilliancy much more uniformly than it can be softened, by laying on more smoke on the darkening glasses. And as few instruments in common use are so large as that to which this method of darkening has been adapted, he expresses his hope that it may be of general use in solar observations.

In the second paper on the *refrangibility of the invisible rays of the sun*, the author first describes his apparatus, which is delineated in Plate XIV. where A. B. represents a small stand covered with white paper, upon which are drawn five lines parallel to each other, at half an inch distance asunder, but so that the first is only a quarter of an inch from the edge. These lines are intersected by three others at right angles, the second and third of which are respectively at  $2\frac{1}{2}$  and 4 inches from the first; 1, 2, 3, represent the thermometers



meters used in the former experiments, mounted upon their small inclined planes, and placed so that the centres of the shadows of their balls fall upon the intersection of these lines. C D is the prism at the window, and E is the spectrum thrown upon the table, so as to bring the last quarter of an inch of the red colour upon the stand. In this arrangement all the spectrum, except the vanishing last quarter of an inch, passed by the edge of the stand, and falling upon the table, could not interfere the experiments. The room was darkened.

The trials of heat were made by causing the spectrum to fall either upon or opposite the outer thermometer, while the other two were kept as standards out of the plane of the refraction; and when it was judged necessary, in repetitions, the thermometer, which had been subjected to the heat, was changed for one of the others. In three experiments with the same thermometer, the heat produced—by rays affording no illumination, but falling half an inch beyond the extreme confine of the red colour; was  $6\frac{1}{2}$  degrees in 10 minutes;—by rays falling one inch beyond that confine, it was  $5\frac{1}{4}$  degrees in 13 minutes;—and by rays falling  $1\frac{1}{2}$  inch beyond that confine, it was  $3\frac{1}{8}$  degrees in 10 minutes. At the other extremity of the spectrum there was no augmentation of heat produced beyond the confine of violet rays. The distance of the prism was 52 inches.

Hence it followed clearly that there are rays coming from the sun which are less refrangible than any which affect the sight; and that they are invested with a high power of heating, but with none of illuminating bodies, which explains the reason why they have hitherto escaped unnoticed.

As the heat was before found not to correspond with the measure of illumination in the spectrum, and in these experiments it extended beyond that limit, it became an object of interest and importance to ascertain the place where the calorific power is greatest. *This maximum of heat was found by experiment to be about half an inch distant from the boundary of the red colour*, and the heat at one inch was fully equal to that of the middle of the red colour itself. The boundaries of what may be called the calorific spectrum lie between the extreme of violet to an undetermined spot at least  $1\frac{1}{2}$  inch, or  $1\frac{1}{2}^{\circ}$  beyond the boundary of the red colour.

In his concluding summary, besides some remarks and recapitulation, which are included in the preceding account, the author adds, that if we may infer the quantity of the efficient from the effect produced, the invisible rays of the sun probably far exceed the visible ones in number; and that if we call *light* those rays which illuminate objects, and *radiant heat* those which heat bodies, it may be inquired, whether light be essentially different from radiant heat? In answer to which he suggests, that we are not allowed, by the rules of philosophizing, to admit two different causes to explain certain effects, if they may be accounted for by one. A beam of radiant heat, emanating from the sun, consists of rays that are differently refrangible. The range of their extent, when dispersed by a prism, begins at violet coloured light, where they are most refracted, and have the least efficacy. These calorific rays have been traced throughout the whole extent of the

prismatic

prismatic spectrum; and their power was found to be increasing, while their refrangibility was lessened as far as to the confines of red-coloured light. But their diminishing refrangibility and increasing power did not stop here; for they have been pursued a considerable way beyond the *prismatic spectrum*, into an invisible state, still exerting their increasing energy, with a decrease of refrangibility up to the maximum of their power; and have also been traced to that state where, though still less refracted, their energy, on account, as may be supposed, of their now failing density, decreased pretty fast; after which the invisible *thermometrical spectrum* soon vanished.

If this be a true account of solar heat, for the support of which he appeals to his experiments, he concludes that we must admit that such of the rays of the sun as have the refrangibility of those which are contained in the prismatic spectrum, by the construction of the organs of sight, are admitted under the appearance of light and colour; and that the rest, being stopped in the coats and humours of the eye, act upon them, as they are known to do upon all the other parts of our body, by occasioning a sensation of heat.

## VII.

*Additional Experiments on Galvanic Electricity. By Mr. DAVY, Superintendant of the Pneumatic Institution.*

TO MR. NICHOLSON.

S I R,

THE earlier experimenters \* on animal electricity noticed the power of well burned charcoal to conduct the common galvanic influence.

I have found that this substance possesses the same properties as metallic bodies in producing the shock and spark † when made a medium of communication between the ends of the galvanic pile of Signor Volta.

I have likewise found that perfectly well made charcoal, when connected with water or aqueous solutions in the galvanic circuit, effects changes in them analogous to those produced by metals; but connected with peculiar appearances.

1. Two long and thin slips of dry charcoal were connected with silver wires attached to the ends of a galvanic pile of 60 pieces. The points of the charcoal slips were immersed in a glass of water, at the distance of half an inch from each other; and the globules of air adhering to them being carefully removed, the communication made sure.

In about a minute, particles of gas began to form and evolve themselves round the point of the charcoal connected with the silver side of the apparatus. Near a quarter of an hour

\* The inventor of the galvanic pile discovered the conducting power of charcoal. His experiments were confirmed by Creve and Schmuck. See Pfaff on Animal Electricity, page 48.

† The spark is most vivid when the charcoal is hot.

clapsed,



elapsed, before any gas was produced from the zinc side; the gas that was produced adhered to the charcoal in large globules, and did not pass through the water. As long as the communication was kept up, the silver charcoal gave out gas very rapidly.

2. Reasoning from the common phenomena of the action of red-hot charcoal on water, and on the analogous galvanic facts, it was reasonable to conclude that the gas evolved from the charcoal on the silver side of the apparatus was hydrocarbonate; and that carbonic acid had been produced on the zinc side, which had been chiefly absorbed by the water.

To ascertain if this conclusion was true, two small open tubes, about one-fourth of an inch in diameter, and three inches long, were provided. Into one end of each of them a thin piece of hard and polished charcoal was introduced, and fastened by cement. They were then filled with distilled water, and inverted in a glass containing that fluid; the tops of the pieces of charcoal being made to communicate with the ends of a pile.

The process was carried on for more than fourteen hours; at the end of which time the quantity of gas produced from the charcoal on the silver side was at least fifty times greater than that produced on the zinc side. The tube from the zinc side, with its water and gas, was introduced into a vessel of lime water. On agitation the water became clouded, but the gas was not perceptibly diminished: mingled with twice its bulk of nitrous gas, it gave such an absorption, as denoted that it contained nearly the same quantity of oxygen as common air.

The gas produced from the silver side of the pile did not at all diminish with nitrous gas; twelve measures of it, mingled with eight measures of oxygen, in a detonating tube, and acted on by the electric spark, inflamed and left a residuum equal to rather more than three measures. Lime water introduced to these became a little clouded, and a slight absorption took place. After this absorption, at least two measures and a quarter of gas remained, which, mingled with nitrous gas, gave red fumes and diminution. Hence they evidently contained oxygen.

3. Surprised at these results, from which it appeared that the gas from the silver side of the apparatus held very little charcoal in solution, and required nearly the same quantity of oxygen to destroy it as the inflammable air from the metals, I repeated the experiment, making use of water that had been long boiled, and was yet warm. In this case no gas was given out from the zinc side during the whole of the process, and more than half an hour elapsed before any was produced from the silver side. What was produced, however, gave nearly the same diminution, when fired with oxygen, as common inflammable air, and the residuum produced but a slight precipitate admitted to lime water.

It was easy to account for the deficiency of gas on the zinc side in this process, by supposing that the gas produced in the former experiment was air previously dissolved by the distilled water, and liberated in consequence of the stronger attraction of carbonic acid for that fluid; but as I had before found that in the common galvanic process with the metals, the hydrogen was immediately evolved, even in boiled water, it was difficult to conceive

conceive why such a length of time was required for the production of the inflammable gas. When I introduced charcoal connected with the zinc side, and silver wire connected with the silver side, into boiled water, gas was almost immediately given out from the wire; though when I connected silver with the zinc side, and charcoal with the silver side, no gas was liberated for many minutes.

4. A slip of charcoal was connected with the zinc end, and a silver wire with the silver end, and both plunged into a vessel of lime water. Gas was immediately given out from the silver wire; a few globules only formed round the charcoal: they were apparently covered with a white crust. As the process advanced, white clouds fell from the charcoal, and diffused themselves through the fluid.

5. Two pieces of charcoal were connected with the ends of the pile, and plunged into a strong solution of caustic pot-ash. During two hours no gas was given out from either of the pieces, and no change of colour was perceived in the fluid, though the communication was perfect. When a silver wire was connected with the zinc, and charcoal with the silver, gas was produced from the wire, but none from the charcoal. When charcoal was connected with the zinc, and silver wire with the silver, gas was very rapidly produced round the point of the wire; but not an atom formed round the charcoal.

6. When the slips of charcoal connected with the pile were introduced into solution of ammoniac, gas was given out from the zinc charcoal; but none from the silver charcoal. When silver wire was connected with the silver side, the charcoal being still connected with the zinc side, gas was given out from both, but most rapidly from the wire. These gases were caught and examined. That from the charcoal gave no diminution with nitrous gas. An accident prevented me from examining it by other tests; the gas from the silver equalled twenty times the volume of the other gas, and appeared to be pure hydrogen.

I shall, at present, offer no theoretical conjectures concerning these experiments. The two last will probably lead to interesting conclusions. I am, at this moment, engaged in examining small quantities of solution of pot-ash and ammoniac which have been long galvanised in contact with charcoal: the result of this examination, if at all important, I shall take the liberty of communicating to you at some future time.

I remain,

SIR,

Very respectfully,

Your's, &c.

HUMPHRY DAVY.

*Dowry Square, Hotwells, Sept. 22, 1800.*

SCIENTIFIC



## SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &amp;c.

## BOARD OF LONGITUDE OF FRANCE.

*Prize of Astronomy.*

THE tables of the moon are equally interesting to astronomy and navigation. The most celebrated geometers have assiduously cultivated the theory on which these tables are founded. The most important care of the practical astronomer is to observe all the motions of this planet, without which there would be no truth in geography, and which furnishes the navigator with the surest means of knowing the situation of his vessel, of directing his course, and safely arriving at any determined spot of the globe. In proportion as the Newtonian theory has been investigated, and the greater the perfection to which instruments and the method of observing have been brought, in the same proportion have the lunar tables been improved. Mayer, joining his own researches to those of the geometers of his time, and making choice of those observations which were the most to be depended upon, succeeded in forming tables which, by subsequent comparison with nearly twelve hundred unpublished observations, agreed with the most astonishing precision. Mason, under the direction of Dr. Maskelyne, rendered them still more perfect, by restoring several equations which had been omitted, though pointed out by Mayer, and by modifying the coefficients of all the others.

In spite of all this care, these tables, which in the middle of the century were so accurate, began progressively to lose their exactness. On recurring to the theory, the cause of the error and its remedy were seen. The prize memoirs, which two years ago were sent to the National Institute, and were published in the session of the fifteenth of *Germinal* last, have placed in the clearest point of view both the necessity and precise quantity of the equations lately discovered for the motions of the apogee and the node. Astronomers were not then invited to employ themselves on all the elements which compose the lunar tables; the labour was too much disproportioned to the time appointed for the concurrence. One successful discovery often gives the wish and frequently the means of making another. That which has been so happily performed, has proved the possibility of doing still more, and of procuring for astronomy lunar tables of greater precision as well as durability. Nothing more remains after the fixation of the epochs, the secular movements, and their inequalities, than to discuss anew, by comparison with a great number of accurate observations, the precise quantity of the different equations which enter into the calculation of the moon's place.

This is the problem which the Board of Longitude of France now proposes for solution to the astronomers of all countries. The conditions are:

1. To discuss, by comparison with a great number of good observations, the value of the coefficients of the lunar inequalities, and to give for the longitude, latitude, and parallax of that luminary, formulas still more accurate and complete than those which have been used as the basis of the tables at present in use.

2. To construct, according to these formulæ, tables of sufficient extent for the convenience and certainty of computation.

The prize is six thousand francs (250l.)

The Board of Longitude appoints no term for the decision, but will award the prize to the first piece which shall fulfil the conditions of the program. It declares likewise to astronomers and geometers that it will not cease to solicit the encouragement of the French government in favor of the perfection and theory of the tables of the moon.

The pieces must be sent *au Bureau des Longitude*, Palais National des Sciences et Arts, à Paris.

They must not bear the name of the author, but merely a sentence or device. If the author thinks fit he may send also a sealed billet, containing not only the device, but the name and address of the author, which billet will not be opened unless the piece itself shall have gained the prize.

The prize will be given without any formality to the bearer of the receipt which the secretary shall have given.

(Signed)

DELAMBRE, President.

LALANDE, Secretary.

*Translation of a M. S. Note of Cit. G. Cuvier, relative to Fossil Subjects of Natural History.*

I am engaged on a large work upon the bones of quadrupeds found within the earth. I have already collected a large quantity of these bones as well from France as from foreign countries, and I have proved that several of them belong to species at present unknown to naturalists.

This is certainly the fact, not only with respect to those fossils which were before known, such as the crocodiles or cetaceous animals of Maestricht, the elephants and rhinoceroses of Siberia and Germany, the bears of Anspach, the elks of Ireland; but also with respect to others which were first discovered by me, such as the tapirs of the usual size, and the tapirs of gigantic magnitude of France; the animals intermediate between the tapir and the rhinoceros, three species of which are found buried in the gypsum in the environs of Paris, whose figures accompany this note \*, to convey an idea of them to those persons who may interest themselves in my researches.

\* The engravings, which are five in number, and will undoubtedly compose part of the work of the learned author, could not, for many reasons, be given with this translation.---N.



Several learned men in Germany, Holland, and Italy, who approve the object of my labours, have supplied me with valuable information, accompanied with drawings relating to the fossil bodies which are found in their respective countries, or which are deposited in their cabinets. On account of the war, I have not, till now, had an opportunity of soliciting the like assistance from the philosophers of England; but I hope I shall experience the same liberal assistance from them.

Under this supposition, I would request an account of such fossil bones of quadrupeds, as may be contained in the British Museum, or in other collections in London; whether derived from Great Britain, or from other parts. I am particularly desirous of those which were buried in the mountain of Gibraltar, of which there must be numerous specimens in London. I am ready to make my acknowledgements in return to those who may thus favour me by informing them of every thing which can be interesting to them in this country on the subjects of natural history and comparative anatomy.

I shall be still more obliged if they will, at the same time, send me drawings of these fossils executed in a manner nearly similar to those which I now send as specimens of engraving, and I engage to defray every expence which may be occasioned by such drawings.

It is needless to observe that I shall, in my intended work, publicly acknowledge my obligations to those who may have enriched it by their contributions.

G. CUVIER,

Member of the National Institute,  
and Secretary to the Class of  
Natural Philosophy.

*Au Jardin des Plantes*

*à Paris. 15 Therm.*

*An. 8. (3 Août 1800.)*

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*Hydraulic Engine operating by Mercury.*

The respectable correspondent who has forwarded a description and sketch of an engine for raising water by mercury, has rightly conjectured that the expedient was used before. Mr. Joshua Haskins, at the beginning of the century, made an engine of this kind, which was improved by Dr. Defaguliers, and is described very much at large in the second volume of his *Course of Experimental Philosophy*. The mercury is merely a substitute for leathering, and has less friction: but it has many disadvantages, which are evident from that description, particularly that it cannot with convenience be used when the column or re-acting force is great; and it may also be expected that the mercury would soon be converted into a black dust or oxide, and carried off in the water. I suppose this to have been the cause of its being abandoned.

*Philosophical Transactions of the Royal Society of London for the Year 1800. Part the First. Quarto, 238 Pages, with eight Plates, and a Meteorological Journal. London, fold by Elmsley.*

This part contains:—1. The Croonian Lecture. On the Structure and Uses of the Membrana of the Tympani of the Ear. By Everard Home, Esq. F.R.S. 2. On the Method of determining from the real Probabilities of Life, the Values of contingent Reversions, in which three Lives are involved in the Survivorship. By William Morgan, Esq. F.R.S. 3. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, for the Year 1798. By Thomas Barker, Esq. 4. On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations. By William Herschel, LL.D. F.R.S. 5. A second Appendix to the improved Solution of a Problem in Physical Astronomy, inserted in the Philosophical Transactions for the Year 1798, containing some further Remarks and improved Formulæ for computing the Coefficients A and B; by which the Arithmetical Work is considerably shortened and facilitated. By the Rev. John Hellins, B.D. F.R.S. and Vicar of Potter's Perry, in Northamptonshire. 6. Account of a Peculiarity in the Distribution of the Arteries sent to the Limbs of slow-moving Animals; together with some other similar Facts. By Mr. Anthony Carlisle, Surgeon. 7. Outlines of Experiments and Inquiries respecting Sound and Light. By Thomas Young, M.D. F.R.S. 8. Observations on the Effects which take Place from the Destruction of the Membrana Tympani of the Ear. By Mr. Astley Cooper. In a Letter to Everard Home, Esq. by whom some Remarks are added. 9. Experiments and Observations on the Light which is spontaneously emitted, with some Degree of permanency, from various Bodies. By Nathaniel Hulme, M.D. F.R.S. and A.S. 10. Account of a Series of Experiments, undertaken with the View of decomposing the Muriatic Acid. By Mr. William Henry. (Philos. Journal, IV. 209.) 11. On a new fulminating Mercury. By Edward Howard, Esq. F.R.S. Ibid, IV. Appendix. Meteorological Journal kept at the Apartments of the Royal Society by order of the President and Council.

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#### *Royal Society of Copenhagen.*

The Royal Society of Copenhagen not having received answers to its questions on history, mathematics, and natural philosophy, which were proposed last year, repeat the same questions for the present year.

1. *History*.—What people discovered America, and travelled into that part of the world before the Norwegians? How far did the discoveries of the Norwegians extend in America, particularly



particularly towards the west? Proofs and conjectures derived from authors, or from existing monuments, such as buildings, language, or the traditions of the Americans must be quoted.

2. *Mathematics.* To discover the function of all the quantities which, jointly, determine the magnitude of the effect of the caloric afforded by every kind of combustible made use of in manufactories, whether wood, pitcoal, or charcoal. When the equation is found, it must be determined in the four following cases: 1°. When wood, pitcoal, or charcoal are burnt in a stove or vessel to heat the confined air; for example of a room. 2°. When these materials are used to boil all sorts of liquids over a fire place. 3°. When they are used to harden soft substances, as, for instance, to make bricks. 4°. and lastly, when employed in melting metals.

Supported by experiments, the authors must discover and establish the different equations analytically; in order that by their application, the effect and economy of each of these three kinds of combustibles, wood, coal, and charcoal, may be calculated.

3. *Natural philosophy.* To find by experiments the greatest degree of heat which aqueous vapours, when heated, can communicate to other bodies? Can that part of the water which, in the digester of Papin, is not reduced to vapour by the heat, acquire more than 212 degrees of Fahrenheit's thermometer?

4. The Society having had the satisfaction to crown an answer to its philosophical question for the preceding year, proposes the following question for the prize of the present year.

What new discoveries have been made in philosophy since the time of Plato and Aristotle, in its researches into, and its explanation of, the nature of human knowledge with respect to existing beings?

The present question here, is not to ascertain what progress has been made in those sciences which treat existences, whether corporeal or incorporeal; but it relates to our internal knowledge (which some philosophers call *subjective*) to know what, in general, is the nature and the power of this knowledge, its origin and cause, the principles and reasons upon which this manner of knowing is determined and established, and upon which is founded that truth which is therein found, or imagined to be found. It is expected that an historical exposition should be given, of what has been produced on this subject by the meditations of philosophers since the time of Plato and Aristotle; how much we are indebted to them for new discoveries, or for facts better established and defined; or on the contrary, if it shall appear that philosophy has not made any progress in this respect, it is expected that this should be demonstrated from the history of the dogmas of philosophers.

The answers to these questions are to be written in Danish, in French, or in German, and forwarded with the usual formalities, before the end of June 1801, to Professor P. C. Abildgaard, Secretary to the Society.

*Decomposition of the fixed Alkalis.*

Van Mons, in a Letter to C. de la Metherie, inserted in the *Journal de Physique*, Vol. VII. p. 390, acquaints him, that *Craaner*, an apothecary at Amsterdam, has made an experiment, by which it appears that the alkalis contain carbon. He includes pot-ash, or soda, moistened with water under an inverted glass with oxygen gas. The gas is absorbed, and the alkali becomes effervescent. Moreover, he finds that an alkali, treated a certain number of times (*quelquefois*) with this gas, becomes exhausted of carbon, and can no longer be made effervescent.

V. M. long ago published, that pot-ash is formed during the combustion of hydrocarbon, and during the fermentation of grapes, by the intervention of the azote of the atmosphere. He adds, that when these operations are made in pure oxygen, neither pot-ash, nor the acidulous tartrate of that alkali are obtained; but the wine becomes too sour to be drank. He conjectures, that the fixed alkali may be carbonated azote.

But he mentions two experiments which militate against this theory. In the first he exposed caustic pot-ash, and the red oxide of mercury by fire, to red heat in a retort, and obtained nothing but oxygen gas, nitric acid and water. In the second, he triturated the oxymuriate of pot-ash with crystals of caustic alkali, and poured the mixture into a phial, which he kept exactly closed for three days. At the end of this period he poured muriatic acid upon it, but had no effervescence.

As carbon is not combustible in the air but at a red heat, our author remarks, that if this principle enter into the composition of alkalis, it must be united with hydrogen, unless azote serves as the medium of its union with oxygen.

In the same *Journal* for Messidor VIII. I find a note, page 82, in which it is asserted, that Guyton Morveau, on the 6th floreal, read to the French National Institute a memoir on the constituent principles of the fixed alkalis. He made his experiments jointly with Desormes pupil of the polytechnic school, and their conclusions are,

1. That pot-ash is composed of lime and hydrogen.
2. That soda is composed of magnesia and hydrogen.
3. That lime is composed of carbon, azote, and hydrogen.
4. That magnesia is composed of lime and azote, and consequently of carbon, hydrogen, and azote.

*Separation of Butter.*

Brugnatelli has separated butter from cream without the assistance of oxygen. He pours four parts of hot water to one of cream, and passes the mixture through a strainer of coarse cloth in a closed vessel, in which several strainers of the same kind are stretched. In twenty-four hours the whey has passed the strainers, and the butter is perfectly separated.

J. de Phys. VII. 391.

*Beet*



*Beet Sugar and ardent Spirits.*

The National Institute of France having named a commission to repeat the experiment of Achard on Beet Sugar, Citizen Deyeux has lately made their report, the result of which is, that the best sugar can be afforded at 15 sous per livre, or 7d. per lb. avoirdupois. The Institute therefore invites the Government to form an establishment on a large scale for this important object of economy.

Mag. Encycl. An. VIII. tom. ii. p. 97.

Brandy and arrack are made at Berlin from the refuse of the beet root. Both are highly praised, and the former is on sale in the market.

*Fourcroy's Synoptic Tables of Chemistry.*

The method of exhibiting the whole of a subject, with the relations of its parts to the eye in tables, is of most admirable use for the perspicuity of communication, and its impressive effect on the memory. It seems as if the caprice of fashion, which during the century nearly elapsed was inimical to every appearance of formality, had in a great measure suppressed these valuable helps to learning. The Tableaux Synoptiques of Fourcroy will be not only of the highest service to the individual science of chemistry, but may tend to shew the advantages of position and arrangement in other sciences, which greatly want it. These tables are printed on twelve large sheets, twenty-two inches by thirty, and consequently with the misfortune of two folds in each. Every sheet contains one table, and the author's arrangement is governed by the chemical properties of bodies. The first table contains the generalities of the science, its order, methods, history, divisions, explication to medicine, &c. 2. The second presents (a) the first class of simple, or undecomposed bodies, as light caloric, oxygen and azote, and others according to their order of combustibility, as hydrogen, carbon, sulphur, diamond, and the metals; and (b) these bodies burned or united to oxygen, or the series of oxides or acids, classed by their attractions to the principle of combustion, and the difficulty of decomposing them. 3. The third exhibits first the salifiable bases, namely earths and alkalis; and secondly, the first classes of salts. 4, 5, the fourth and fifth tables are also employed in the classification and description of salts 6, 7, 8, 9, 10. The five following tables are engaged on the twenty one metallic substances hitherto discovered. 11, 12. and the eleventh and twelfth present sketches of the vegetable and chemical chemistry.

These tables may be considered as the synopsis of the *Système général des Connaissances chimiques* of the author, now in the press, and after its appearance he will publish a fuller series of tables on the subjects of the two last.

The present tables are on sale at De Boffe's, in Gerard Street, London.

*Note from a Correspondent respecting the Bolognian Phosphorus.*

TO MR. NICHOLSON.

S I R,

I AM in possession of some stones from Monte Paderno, near Bologna, of which the phosphorus, called there *Spongia di luce*, and *Pietra cuminabile*, is made. I have consulted all the books of chemistry which mention the method of making this phosphorus, and several experienced chemists have exactly followed their directions, but in vain, as no one has been able to cause the stone to imbibe light.

Your inserting this in your valuable Journal, may perhaps be the means of procuring some information on the subject, and the exact method of preparing it; likewise if it can be procured in London. I brought some from Bologna some years ago, but it soon lost its property of retaining light.

I am, Sir,

Your humble Servant,

Sept. 24, 1800.

R. T.

*Extract of a Letter from Mr William Henry, dated Sept. 25, 1800, to correct an Inference in his Paper on Galvanism.*

"The inference which I had drawn, from the experiments described in your Journal for August, respecting the decomposition of the vegetable alkali, I had found to be erroneous, before the interesting facts of Mr. Davy were known to me; and I had fully intended a recantation, along with an account of some other experiments on the same subject, in which I was engaged, above a month ago. These, however, various circumstances have prevented me from completing; and I think it proper, therefore, not to delay making an acknowledgment, and stating the cause, of my mistake.

"The fact which first led me to suspect, that I had drawn too hasty a conclusion, was, that the black precipitate proved, on examination, to be merely a metallic oxide, and not charcoal, as I had supposed probable. Varying, therefore, the circumstances of the experiment, I transmitted the galvanic influence through liquid caustic alkali, without the contact of mercury, and the black powder then ceased to appear. The gases also turned out to be a mixture of hydrogen and oxygen, in the proportions, pretty nearly, that might be expected from water. This oxygen, the imperfect metals, contained in the quicksilver, had before prevented from appearing in a gaseous form. From ammonia, the contact of mercury being excluded, gases were obtained corresponding, in kind and proportion, with those stated in your Journal (p. 261), the quantity of oxygen gas appearing to diminish as the alkaline solution was more completely saturated."



*H. Clovis. Apparatus for raising  
Water by its lateral Action.*

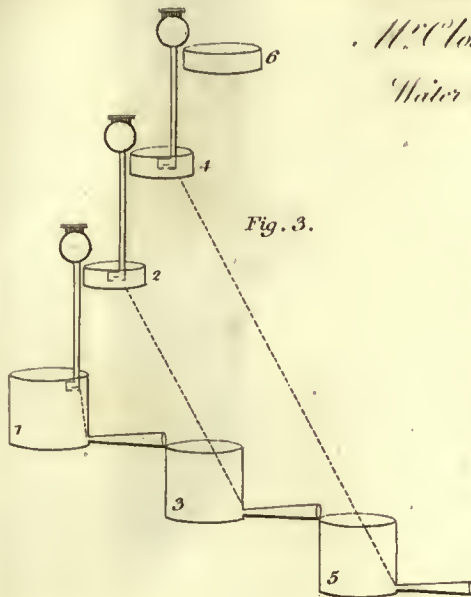


Fig. 3.

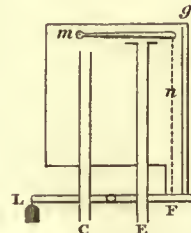


Fig. 2.

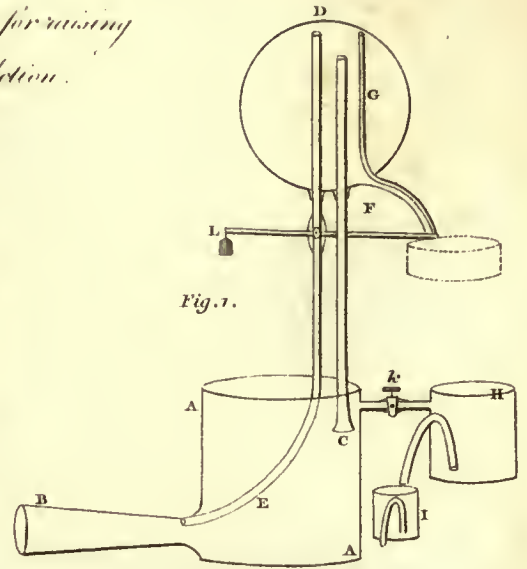


Fig. 7.

*D. Wollaston's explanation of double images caused by atmospheric Refraction.*

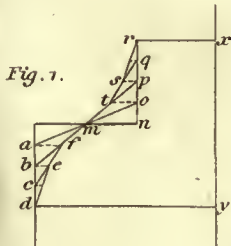


Fig. 1.

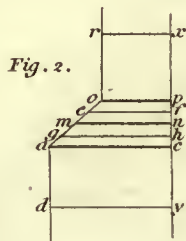


Fig. 2.

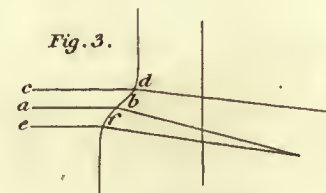


Fig. 3.

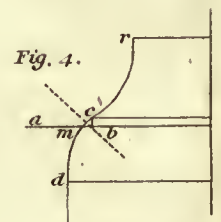


Fig. 4.

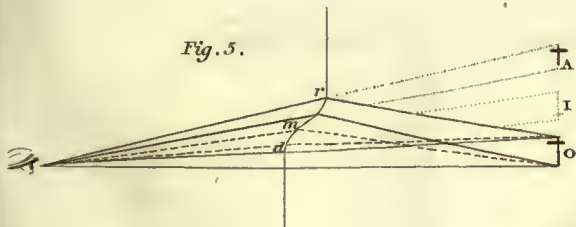


Fig. 5.



Fig. 6.

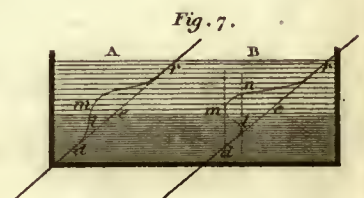


Fig. 7.

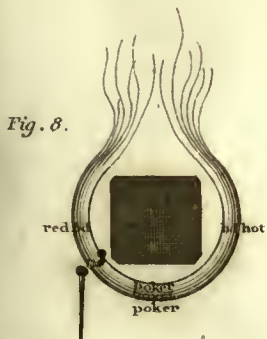


Fig. 8.

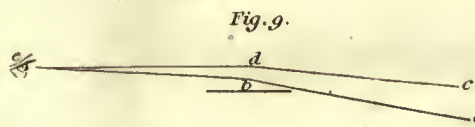


Fig. 9.

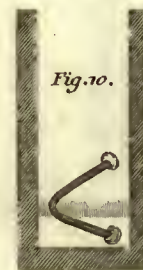


Fig. 10.

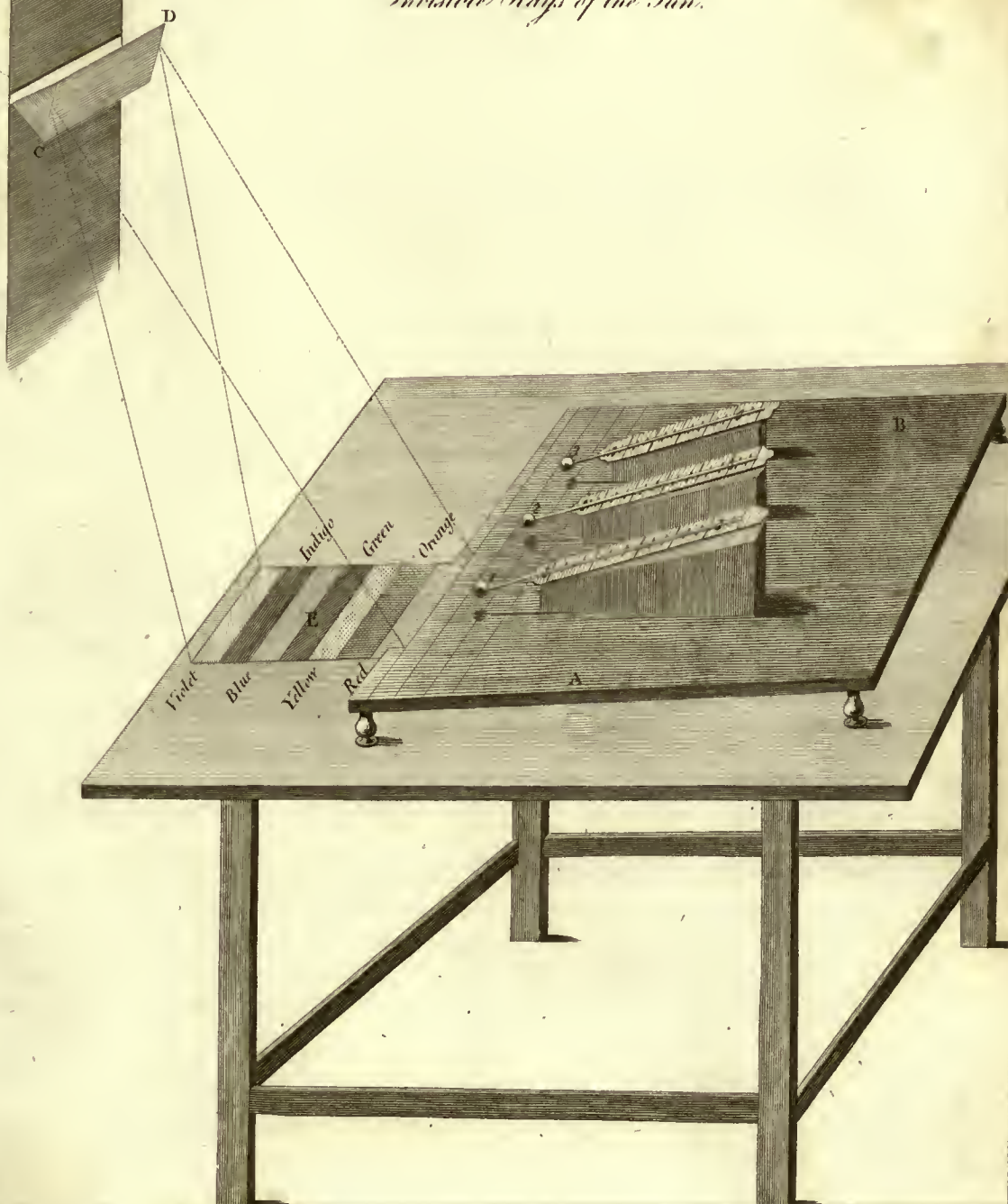


Fig. 11.





*D'Herschel's Experiments on the  
Invisible Rays of the Sun.*







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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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NOVEMBER 1800.

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ARTICLE I.

*Notice of some Observations on the Causes of the Galvanic Phenomena, and on certain Modes of increasing the Powers of the Galvanic Pile of Volta. By Mr. DAVY, Superintendant of the Pneumatic Institution. Communicated by the Author.*

THE nature of this communication is incompatible with a detail of the opinions prevailing amongst philosophers, respecting the causes of the galvanic phenomena: they have been generally supposed to depend on the different powers of bodies to conduct electric fluid; Fabroni was the first who systematically attempted to prove that they were chemical effects\*.

Immediately after I had perused an interesting observation of Lieutenant-Colonel Haldane† on the non-excitement of galvanism in the vacuum of an air pump, I began an investigation with the view of ascertaining precisely the influence of the atmosphere on the phenomena. In carrying on this investigation, I have met with some new facts, which are capable of arrangement, and which will probably lead to a complete explanation of the galvanic effects.

\* Phil. Journal, III. 308.

† Phil. Journal, IV. 242.

The piles that I employed for ascertaining the influence of factitious airs on the galvanic phenomena, were erected horizontally in the usual mode; but to prevent the plates from separating when in an oblique position, their sides were joined together by resinous cement at two or three points, sufficient interstice being preserved to admit of a free circulation of air. The gases, when any were produced, were received in small tubes filled with distilled water, containing wires covered externally with wax, and communicating with the ends of the pile. The piles were introduced into the airs through water\*, and elevated above the water by a metallic plate cemented to their lower extremities.

- 
1. *Zinc, whether connected with Silver in single Galvanic Circles, or constituting the Plates of the Galvanic Pile, seems to undergo no Oxidation at common Temperatures, as long as the Water in contact with it is pure.*

By pure water, is more immediately meant water holding in solution no oxygen gas, no nitrous gas, and no acids. It has long been known that certain metallic bodies, which oxidate slowly in water exposed to the atmosphere, effect no change in pure water†: this I have observed is particularly the case with regard to zinc. That zinc, when in contact with silver in the atmosphere, and forming with it a circuit by means of water, becomes oxidated much more rapidly than when simply in contact with water, was observed by Dr. Ash‡.

Supposing the more rapid oxidation to be the effect of a peculiar electrical influence produced by the contact of the metals, it would be reasonable to conclude, that zinc in single circles with silver and pure water, or at least in the galvanic pile erected with cloths moistened in pure water, would undergo oxidation. Fabroni§, however, has advanced that single galvanic circles do not oxidate in water, unless it be exposed to the atmosphere. That the zinc of the galvanic pile does not oxidate in contact with pure water, will appear from the following observations:

a. A small pile of silver and zinc, having its pasteboards moistened with water, that had been just boiled, was introduced into a vessel of water that had been long boiling, and was yet warm. Resinous cement was poured upon the surface of the water, and fastened to the glass as it cooled, to preserve it effectually from the contact of air||. The apparatus, after remaining for two days, was examined; the zinc plates were scarcely at all tarnished; no oxide was deposited in the water, and no gas had been evolved through it. A similar

\* Before these experiments were made, I had found, by numerous trials, that a pile acted in the atmosphere immediately after its immersion in water without being wiped, though more feebly than before: I had likewise found that after the first immersion, the powers were not diminished by subsequent ones.

† See Fabroni, Phil. Journ. III. 309.

‡ And Humboldt, see Researches Chem. & Phil. page 568.

§ Phil. Journ. III. 309.

|| Oil of turpentine, and even common oil, as will be seen hereafter, is ineffectual for this purpose.



pile, exposed for nearly the same time to water in contact with the atmosphere, had deposited much white oxide, and given out some gas, and the zinc plates were whitened both internally and externally\*.

b. No influence can be communicated from an elastic atmosphere, enabling the zinc of the galvanic pile to decompose pure water; for piles were introduced into hydrogen, nitrogen, nitrous oxide, and hydro-carbonate, and suffered to remain in them for different lengths of time; but in none of these gases was the zinc more oxidated than if the pile had been immersed in pure water.

c. A galvanic pile was suffered to remain in vacuo for fourteen hours, the gage being about  $\frac{1}{10}$ , the zinc plates were slightly tarnished, but no white oxide had formed upon them.

2. *The Oxidation of the Zinc Plates of the Galvanic Pile takes place whenever the Water in contact with them holds Atmospheric Air, or Oxygen, or Nitrous Gas, or Nitrous Acid, or Marine Acid, &c. in Solution.*

a. The oxidation of the zinc of the pile in the atmosphere, and the protusion of carbonate of soda, has been accurately described by Messrs. Nicholson and Carlisle. I have found that the phenomena of oxidation takes place much more rapidly in pure oxygen than in the atmosphere. I have likewise found that it takes place more slowly in nitrous gas than in the atmosphere. That zinc is oxidated in solutions of nitrous and marine acids, is a well known fact.

3. *When the Zinc in contact with Water, holding in solution Substances containing loose Oxygen, or Acids, is oxidated, these Substances are altered, or they exert some Chemical Affinities.*

a. A small galvanic pile, exposed to about twenty-one cubic inches of oxygen gas for six hours, effected a diminution of it which might have equalled one-fourth.

b. A similar pile, exposed to an equal quantity of atmospheric air for two days, diminished it one-sixth: the residuum being suffered to pass into the atmosphere, gave a smell which strongly resembled that of ammoniac; and the pile held over marine acid produced dense white clouds.

In another experiment, an equal quantity of air, exposed to the pile for three days, gave but a very slight diminution with nitrous gas, and could not be inflamed with atmospheric air.

c. I have noticed, *Ref. Chem. & Phil.* that wetted zinc, placed in contact with nitrous gas over mercury, slowly converts it into nitrous oxide and ammoniac. A small pile, ex-

\* The temperature in these experiments was from  $54^{\circ}$  to  $61^{\circ}$ . It is impossible to free water of all its dissolved air by boiling: it is likewise impossible to prevent a few globules of air from passing through the cement whilst it is cooling: the very slight tarnish of zinc kept in boiled water may be owing to the minute quantities of common air existing in the water from both these causes.

posed to about twenty-two cubic inches of nitrous gas for three days, diminished it one-fourth, and some of the gas was rendered absorbable by water.

*d.* The formation of nitrous gas, nitrous oxide, and ammoniac, when very weak solutions of nitrous acid are made to oxidate zinc, demonstrate both the decomposition of the acid and the water.

*e.* The oxidation of zinc in solutions of marine and sulphuric acids in water, appears to be owing to the affinity which has been called predisposing.

4. *The Galvanic Pile of Signor Volta seems incapable of acting when the Water between the Pairs of Plates is pure.*

The word *pure* is here used in the same sense as in 1.

*a.* I have found, by numerous experiments, that the galvanic pile introduced into hydrogen, through common water, ceases to evolve gas in the tube, and to act in about five or six minutes, *i. e.* in about the time required to consume the atmospheric air dissolved in the common water between its plates. The phenomenon is exactly the same in nitrogen, nitrous oxide, and hydro-carbonate. The action of a pile, as known by its power of evolving gases from water, is diminished immediately on its introduction into those gases. It ceases in them nearly in the same time, and cannot be restored by admitting fresh gases of the same kind, though, as will be seen in the next section, it is immediately restored by immersing the pile for a moment in water saturated with atmospheric air.

*b.* I have found, by numerous experiments, made with the assistance of my friend, Mr. King, that the galvanic pile ceases to act in vacuo when the gage is at about  $\frac{6}{40}$  of an inch, even though the poles are connected by wires with the atmosphere, and the water it is made to act upon in contact with the atmosphere.

5. *The Pile acts when the Water between the double Plates holds in Solution Atmospheric Air, or Oxygen, or Nitrous Gas, or Nitrous Acid, or Marine Acid.*

This fact I have proved by numerous experiments.—*a.* A pile acted in atmospheric air, included in a glass cylinder over water for two days, till nearly all the oxygen of the air was consumed.—*b.* A pile decomposed water much more rapidly in oxygen than in common air, and less rapidly in nitrous gas than in common air.—*c.* That the influence of an elastic oxygenated atmosphere is not essential to the galvanic effects, is evident from the fact of the constant restoration of the powers of a pile after they had ceased to appear in the hydrogen, nitrogen, &c. by momentary immersion in water saturated with atmospheric air. In these experiments the piles were plunged into the water confining the gases, and again immediately elevated into the gas without being exposed to the atmosphere: and the phenomenon could be owing to no other cause than the impregnation of the water with atmospheric air, because when piles were plunged into water saturated with nitrous oxide\*,

\* This gas expels much common air from water; see *Research. Chem. & Phil. R. II. S. 1.*



their powers were not restored.—*d.* I have proved by many experiments \* that water, deprived of air, is capable of attracting it from the spirit of turpentine when the last is in contact with the atmosphere. The galvanic pile acts in spirits of turpentine for a great length of time, and nearly as well as in the atmosphere, the water between its plates being constantly supplied with air from the spirits. It acts but for a short time in spirits of wine, on account of the combination of this fluid with its water.—*e.* After a pile had ceased to act in hydrogen, its powers were uniformly restored by a momentary immersion in very diluted marine acid. They were likewise restored, and rendered more intense than in the atmosphere, by momentary immersion of the pile in diluted nitrous acid, though they continued to be exerted for a short time only, *i. e.* till the acid was decomposed or saturated.

6. *The Power of Action of the Pile of Volta appears to be proportional to the Power of the conducting fluid Substance between the double Plates to oxidate the Zinc.*

This seems sufficiently proved from the facts in the following sections. The zinc oxidates less rapidly in nitrous gas than in atmospheric air, and less rapidly in atmospheric air than in oxygen: and the power of action of the pile as known by its evolving gas from water is greater in oxygen than in atmospheric air, and greater in atmospheric air than in nitrous gas. The power of the pile to decompose water, and to give the shock is wonderfully increased after it has been dipped in marine acid, and still more increased after it has been dipped in weak nitrous acid; and these bodies only enable the zinc to oxydate itself more rapidly. A series of plates, in which the oxidating conducting fluid was strong nitrous acid, acted, as will be seen hereafter, infinitely more powerfully than any other combination; so that it would seem that the power of a pile is not much connected with the evolution of hydrogen from water.

#### 7. *Conclusions.*

Of two phenomena, or of two series of phenomena, we can only affirm that the one is the cause of the other when it uniformly precedes it, and when their modifications are connected. But it appears from all the foregoing facts, that the galvanic pile of Volta acts only when the conducting substance between the plates is capable of oxidating the zinc; and that in proportion as a greater quantity of oxygen enters into combination with the zinc in a given time, so in proportion is the power of the pile to decompose water, and to give the shock greater. It seems therefore reasonable to conclude, though with our present quantity of facts we are unable to explain the exact mode of operation, that the oxidation of the zinc in the pile and the chemical changes connected with it are *some how* the cause of the electrical effects it produces.

\* These experiments will be hereafter detailed.

8. *Of a new Mode of constructing a Pile.*

Assuming the truth of this conclusion it was easy to conceive, that a pile much more powerful than any hitherto constructed might be made, particularly supposing that the decomposition of water was not essential to the process, plates of zinc and silver, 1.2 inches square, were fastened in pairs by resinous cement: eighteen of these pairs were connected to each other by cement, and so inclosed by it as to leave water-tight partitions open at one side only between each pair of plates. When muriatic acid was poured between the partitions of this machine, the plates being perpendicular, it acted very powerfully; its capability of decomposing water and giving the shock, being at least equal to that of a common pile of seventy plates. Diluted nitrous acid made it act still more powerfully. When the partitions were filled with water, its action was barely perceptible. Concentrated nitrous acid was poured into them. In this case the first shock was so powerful as to benumb my fingers for some seconds, and I did not dare to take another. I was almost immediately obliged to throw the pile into water to prevent it from being destroyed, so that there was no time to ascertain its power of decomposing water.

In a second experiment, with strong nitrous acid, I used only five pairs of plates, when the shock was full as powerful as from the common pile of thirty plates.

Three pairs of plates, with nitrous acid, gave a very sensible shock.

I have procured (on account of the loss of the silver when this substance is part of the pile with nitrous acid) a number of plates of copper, iron, and zinc. They have not yet been disposed in the apparatus; but I expect by means of nitrous acid, to produce effects from them, equal to those of the strongest electrical battery.

## II.

*On raising Water by the Engine \* of H. GOODWYN, Esq. through double or treble the Space of the descending Column, and on the proper Arrangement to make it require no Attendance. By a Correspondent.*

TO MR. NICHOLSON.

S I R,

AFTER considering various methods of raising water with little trouble and expence, I am decidedly of opinion that Mr. Goodwyn's engine will answer this purpose the best. It is formed upon a very elegant principle, and operates by the assistance of only a small quantity of water: it may be made in various forms, either to raise the fluid above the descending column, or from below it to a level with the bottom; and the height may be

\* Described by the inventor, in this Journal IV: 163.



doubled or trebled, by proportionably encreasing the descending mass, and raising several columns of water from different elevations at the same time.

To extend therefore the utility of the invention by illustrating what has been here premised is the intention of the present communication.

A and B, in Pl. XV. Fig. 1. represent the vessels which contain the rising and descending bodies of water: they are spherical in the original drawing, (Pl. VIII.) but to lessen the loss of space in descent, they are here made flat and cylindrical.

E the higher, and C the lower cistern of the original figure.

F, a vessel the same as A, with tubes 1, 3, and 6; it communicates with the vessel B, by the tube 1, and is intended to raise water out of the cistern E into a higher and additional cistern G.

2. enlarged tube of the original drawing.

5. hole in the top of B, instead of a tube. This hole and the tubes 2, 4, 6, 6, must be provided with valves, all which, while water is rising must be kept close by weights or springs, except the valve to tube 2, which must be open.

The tubes 3, 3, may also have valves to support the raised columns.

To make the engine operate without attendance, a vessel containing a syphon must be fixed to the side of the cistern C, and supplied with a proper quantity \*. Every external valve, except the lowest, must have a lever joined to it; and while closing their respective tubes, they should be connected together, and to the lever of the valve to pipe 4, which must suspend another empty vessel and syphon.

The valve of pipe 2, must also be suspended by a chain or wire from the underside of valve to pipe to 4, and when one closes the other must open.

Fill now the cisterns C and E with water, and let the lower be constantly supplied: the vessel on the outside will soon become full, and the syphon will empty the water into the lower vessel, which, by its weight will open the valves of the tubes 4, 5, 6, 6, and close the valve of tube 2: the vessel B, being filled through the hole 4; the descent of water through the higher syphon having ended; and the lower vessel also being nearly emptied by its syphon; the valves will close the tubes 4, 5, 6, 6, and the valve of tube 2 will open; the water will then begin to descend out of B, and raise one body of water out of C into A, and another out of E into F. When B is nearly empty, or A and F are full, water will again descend through the syphon of the higher vessel into the lower, which will open the tubes 4, 5, 6, 6, and close 2: B will fill a second time, and the vessels A and F will empty themselves into their respective cisterns E and G. And thus the reciprocations continue without interruption.

Another body of water may be raised out of G into a higher cistern by additional apparatus, and by proportionably encreasing the dimensions of B and the tube 2.

\* The author evidently means to refer to the papers of Mr. Boswell and Mr. Close, at pages 117 and 293 of our present volume.

The dotted lines represent apparatus for raising water below the bottom of tube 2, to be used instead of those above the cistern C.

This arrangement places the utility of the engine in a very striking point of view, particularly when we wish to employ a small stream to operate upon machinery; where the descent is great, but the quantity of water inadequate to the purpose even with the assistance of a reservoir. In this case let nearly the whole of the water descend through the vessel B and tube 2, before it enters the reservoir; and let the water after it has turned the wheel be collected in a lower basin. If the descent from the higher reservoir be so great as to allow part of the lower body to be raised into it again at once, the quantity raised may be made nearly equal to that which has descended through the tube 2; if it can be done at twice, it will be less than half that quantity. Still however I think it will be very advantageous to employ such a method, as the operation of the engine is constant and diminishes no power of the machinery\*.

In situations where this machine can be erected it may be of considerable use, for raising water out of mines, for draining pieces of ground, or elevating the water employed in domestic purposes.

I am Sir,

Your humble Servant,

Sept. 16, 1800.

L.

### III.

*Observations and Experiments on Light and Heat, with some Remarks on the Enquiries of Dr. Herschel, respecting those Objects. In a Letter from Mr. JOHN LESLIE.*

TO MR. NICHOLSON.

S I R,

London, Oct. 9, 1800.

I AM induced to trouble you with this communication in consequence of the account inserted in your last number, of two papers lately read at the royal society. Aware that the celebrity which their author has so justly acquired as a skilful and indefatigable observer will, from the indolence of our habits, confer a certain extrinsic weight, even when he embarks in philosophical speculation,—I cannot help feeling, anxious, lest *that* authority should in the present instance retard the progress of science by giving currency to opinions which, I am fully convinced, are inaccurate or unfounded. It is not my intention to censure the efforts of genius; but I would suggest how liable are men of talents to commit mistakes in matters

\* The reasoning in this paragraph appears to be defective, because the machine engages part of the fall, and thus diminishes the effect more than the raised water can augment it.



of experiment, especially if they disregard analogy and the general concurrence of established facts. It would appear that this able astronomer, on entering into a new line of enquiry has neither employed apparatus suited to the nicety of the subject, nor guarded sufficiently against the numerous and latent sources of error. I consider myself entitled to speak with the greater confidence, because I had long directed my researches in the same channel, and had fortunately hit upon a simple contrivance, improved since by persevering attention which afforded to such investigations a degree of precision and facility not hitherto attained. I do not hesitate, therefore, to maintain that Dr. Herschel's capital proposition originates in fallacious observations. To support this assertion I will briefly and with reluctance state the result of my enquiries, and will then offer some remarks or strictures on the article in question.

In describing my *photometer* I noticed, in a cursory manner, the several points in optics and chemical philosophy for the examination of which it was, by its peculiar delicacy, so happily calculated. It was not then my design to enter into details, or relate the success of the experiments. Those interesting deductions I reserved for a work which has been several years in contemplation, but of which I have deferred the publication from time to time as the materials accumulated, from a desire still to extend and correct my views, and perhaps from a growing indifference to the voice of public opinion, which I am sorry to observe is, in this country at least, notoriously servile and undiscerning. Nor will I at present anticipate more than what immediately concerns the subject under discussion.

After constructing the instrument one of my first objects was to determine, by their calorific effects, the relative intensities of different colours. The problem had indeed been attempted before, by Franklin, Watson, and others; but from such rude trials nothing in any degree accurate could be expected. I therefore coated the absorbent ball of a photometer successively with a series of water-colours, and, after allowing it to dry, I placed it with another photometer in the same steady light, whether exposed to the diffusive influence of the sky, or to the direct impulsion of the solar beams. The ratio of the quantities marked by the two instruments gave the proportional absorption of the coloured substance, and consequently its power of reflection. Instead of paint, I sometimes covered the ball with bits of coloured silk; and when two colours showed nearly equal effects, I had recourse to an expedient which rendered their difference very sensible. I made the balls of the photometer larger than usual, brought them to the same elevation, and bent them back in opposite directions; the glass-case consisted of the larger segment of a hollow sphere surmounting a cylinder; the zero of the scale was placed about the middle of one of the branches, and the divisions were marked both in ascent and descent: on applying the approximate colours to those counteracting balls the difference, whether in excess or defect, of their absorbent powers was easily determined. Those experiments were likewise varied by reflecting the rays of the sun, at a given angle from a coloured surface, against the dark ball of a photometer. I need not descend to particulars, but I was struck with the copious reflection from scarlet, little inferior to that from a white surface. Blue, and next to it green, reflected

the smallest quantities of light. Hence the explanation of several facts which occurred in the application of the photometer. Exposed to the sun-beams only, it indicated about one hundred degrees at noon during the summer months in our climate; sheltered from those direct rays it gives seventy or eighty degrees, when the sky is covered with fleecy clouds, but only ten or fifteen degrees if the atmosphere is clear and of azure hue. On the summits of elevated mountains, the sun would be more resplendent while the vault of heaven, of the deepest blue, would emit a proportional feeble light. The red gloss of a fire was found to make at least three or four times as much impression as the flame of a candle, under the visual angle. And language, that grand monument of human thought and feeling, testifies the conformity of those remarks with common sentiment. Red and orange are termed *warm* tints, and we admire the *glow* of the Italian landscape. Among the colours scarlet is reckoned glaring, green, soft, and blue, faint. The pleasurable sensation excited by the sight of green, independent of its association with verdure and vegetable life, has been referred to the circumstances of its occupying the middle rank of the system of colours. But green has more affinity to the dark and absorbent shades, and the eye seems more gratified with deficiency than excess of light.

On a hasty trial, I perceived that, the coloured spaces of the prismatic spectrum manifested analogous properties; but as the quantities were small and required nicer observation, and as the season was already far advanced (it was in October 1797), I resolved to postpone the whole till the next summer. I prepared accordingly to resume the investigations with every advantage; but I am sorry to say that my plan was not accomplished so soon as I expected, owing partly to our variable sky, so discouraging for optical experiments, and partly to various unforeseen interruptions, especially that of undertaking a tour through the north of Europe. In the beginning of last summer, however, my wish was realized, yet not without a tedious waste of time; for such is the nature of this insular climate, that the sky at mid-day is seldom cloudless, and, even then, its azure is generally adulterated by a milky haze. I cut a broad horizontal slit in the window-shutter, sufficient to admit freely an excellent prism of flint glass, which was fixed with wedges at the ends in a position to produce the greatest refraction. Behind was placed a stand about  $1\frac{3}{4}$  inches broad,  $\frac{3}{4}$  of an inch thick, and of the height of 5 or 6 feet, to which an arm projecting 2 or 3 inches from the edge was adapted to slide upwards or downwards at pleasure, and to this the photometer was attached; and, to avoid as much as possible every chance of deception, I annexed to that arm a slender branch stretching three or four inches towards the window, and bearing a small screen capable of being raised or depressed as occasion required, so as to intercept at a distance the whole of the coloured rays but those submitted to examination.

I shall omit mentioning other precautions used; to those who are acquainted with accurate experimenting it would be superfluous, and to the rest of your readers it would appear disgustingly minute. The stand was approached as near to the window as was compatible with the development of the several coloured spaces, which was at the distance of about two feet;



feet; and though the shutter was closed, sufficient light entered by the edges of the prism for observing the instrument. In two or three minutes, as the colour travelled along the black ball, the fluid generally attained the point of equilibrium. Photometers of both constructions were employed with similar results. The observations were frequently repeated, and from the mean of them all it appears; that, distinguishing the spectrum into four equal portions, which coincides with the blue, the green, the yellow, and the red the corresponding intensities or calorific measures, are 1, 4, 9, and 16 degrees, as in fig. 2. Pl. XV. Hence the gradation of those intensities will be denoted by a parabola referred to its vertical tangent. A variety of curious propositions may hence be deduced. Thus, since the complimentary space of the parabola is the third part of the circumscribing rectangle, it follows that the intensity of the red rays separated by a prism of flint glass is triple that of the compound white beam; and for the same cause the blue rays are more than five times feebler than the common standard. It is not without reason that I state the particular sort of prism used, because with one of another substance the colours might be expanded after proportions materially different, which would of course give different results; for the force of a pencil of coloured light must evidently depend on the density, as well as on the species of the rays.

The important discovery of the achromatic telescope decisively evinces that refrangibility is not a property inherent essentially in the particles of light, but results from the specific relation or affinity between those various particles and the refringent medium. In what proportions the several rays enter into the composition of the solar beam has not been determined, nor does any mode of solving the problem easily occur.

It is remarkable that all what is really valuable in Newton's optical discoveries was announced at an early age, but encountered such opposition or experienced such neglect, as to affect that great man with chagrin, and to extinguish completely the desire of communicating his views at large to the world. Nor did he resume that task till near the close of life, when worn down with infirmities, and his choicest faculties wasted in the composition of laborious works, which, for his credit with posterity, had better been suffered to rest in oblivion. Yet even these (such is the effect of exalted reputation!) were now accepted with indiscriminate applause. Had his treatise of optics been published at an earlier and happier period, it would certainly have been more sober and correct. The septenary and musical division of the prismatic spectrum is devoid of foundation, and too plainly betrays a tincture of the mysticism of the age. It is equally strange and mortifying to observe the most objectionable part of that system, and which is directly confuted by the theory of achromatic glasses, still repeated in popular books, and even admitted by some authors of a higher class.

While I was occupied with those observations in Fifeshire, some vague accounts reached me of a paper communicated to the Philosophical Society at London. And whatever my sentiments were respecting the validity of the conclusions, I resolved calmly and impartially to subject the pretended facts to the test of experiment. When a pho-

tometer was placed beyond the spectrum, though it approached the margin, whether above, below, or at the sides, no effect whatever was perceived. - And as that instrument greatly surpassed, in sensibility and regularity of performance, the finest thermometer, it is only what might be expected, that I could not perceive any alteration, if I employed thermometers of uncommon delicacy and of various constructions, with black or pellucid bulbs, having their scales subdivided into tenths of a degree, and capable of indicating still smaller differences. There is a circumstance, however, which being overlooked, might lead to egregious errors. If the spectrum be received on the stand, the instrument brought near it will be very sensibly affected; owing partly to the light reflected, but chiefly to the action of the heated air accumulated over the illuminated surface. In short, the operations are deranged in every case where the instrument is not completely insulated, and is remote from all solid materials which might detain the light. And it is worthy of remark, that the afflux of light must raise the temperature of a flat surface of imperfect conducting substance, such as wood or pasteboard, more than four times as much as it will the blackened ball of a thermometer; for, in the former case, the heating and cooling causes are exerted on the same spot, but in the latter, the quantity of light or heat received is only as the central section of the ball, while the whole surface, which is quadruple that space, is exposed to the cooling influence of the ambient air. There are other circumstances, too, which tends to augment that difference of effect, but which I shall not stop to mention. On the whole it is most difficult, where an active source of heat exists, to obtain an uniform temperature, particularly in a small room; and I might point out causes, not yet apprehended, which will notably affect a thermometer.

These hints will serve as a clue for detecting Dr. Herschel's mistakes. The first circumstance that begets suspicion is the large quantities marked by his thermometers, and which are not much inferior to the full force of the sun-beam. To procure the colours of the spectrum distinctly evolved, the solar image must be extended to ten times its breadth; nor can it be imagined that rays so attenuated could produce such palpable effects. The Doctor, indeed, admits that a small thermometer was less affected. Yet it is obvious that, if the action was due merely to the afflux of light, the size of the ball ought to occasion no alteration, since, if it presents less surface to the beam, it likewise, in the same ratio, exposes less surface to the surrounding air. A very curious reason is assigned—"the *cooling* agency of the *ascending* stream of air, which would act more strongly where the proportion of surface to bulk was greater\*." The idea of an  
*ascending*

\* I must in fairness take notice that the words here quoted from Philos. Journal IV. 321, are not Dr. Herschel's but mine, as the abridger or narrator of the contents of his paper. The Doctor's observation (Philos. Transf. Y. 1800. page 261,) states in very general terms that the *cooling causes* may, he supposes, have a stronger effect on the small than the large thermometer. I do not here quote more fully, because the point seems not of such importance as to demand it, and because the only mistake respecting it appears to lie with the ingenious author of the present communication. His second reflections will not, surely, permit him



*ascending cold stream* is certainly original; at least, it stands in direct opposition to all the established principles of philosophy, though it is not calculated to inspire the most favourable opinion of the author's acumen in physical inquiries. The tardy progression from violet to red compared with the numbers which I have assigned, seems to betray the accession of extraneous influence. Besides, the dark ball of a thermometer will, for the most part, receive its full impression in the space of one or two minutes; but the Doctor's thermometers required, or at least were allowed, an exposure of ten or thirteen minutes. This fact affords a strong presumption that other matters were brought into action, capable of a more prolonged absorption, and of a greater accumulation of heat, than a mercurial bulb. Nor can I hesitate to impute the principal derangement to the platform on which the thermometers were laid, which, receiving light, though partially, would, as before observed, acquire heat in a quadruple ratio, and communicate this to the contiguous stratum of air. The same consideration will explain the origin of the mistake on which the paradoxical assertion is grounded, of "invisible refrangible light." Indeed, a more objectionable plan of conducting experiments could scarcely have been devised. The prism was fixed in an inverted position, to direct the rays downwards upon a table, which was covered with white paper; and the thermometer was placed a little beyond and above the edge of the red light. In such a situation no wonder that the bulb was notably affected, being immersed in a warm atmosphere which extends to a certain distance over the illuminated space, and receiving likewise a large portion of the several coloured rays reflected from the paper. That joint effect would be still further increased by the thermometer having its bulb directly opposed to the ascent of the heated air. The action might even be greater than if the bulb were placed in the course of the red rays, since it would then intercept that light which would have produced an accumulated effect on the wood below. But why is the *maximum* of invisible light stated at half an inch beyond the red rays? Does it move in straight lines? Does it diverge at a certain angle, or has it various degrees of refrangibility? These were questions to be resolved. And after all, what has *invisible* matter to do with that peculiar structure of the surface of bodies which constitutes black or white?

Should any doubt remain after this long examination, there is a single fact which at once demolishes the whole fabric. If those invisible calorific rays had any real existence, the action of a burning glass would be most powerful, not at the proper focus, but a considerable way beyond it. For the same reason, the hole burnt in a black piece of cloth would not be confined to the lucid focus; but would include a ring swelling all round to more than double that diameter.

him to deny that every current of air which ascends, in consequence of the proximity of an heated body, must be cold when compared with that body itself; or (to keep more strictly to the expression alluded to) that it cannot receive the heat which makes it ascend, without cooling, or exerting a cooling agency on, the body which heats it. N.

I have already extended this paper beyond the limits I first proposed: I will not, therefore, fatigue your readers by attempting the refutation of some obscure and verbose arguments advanced by Dr. Herschel, which involve a singular species of logic, and seem repugnant to the fundamental principles of dynamics. The hypothesis of invisible light is not new in the scientific world. It was suggested by the radiant heat of Scheele and the radiant cold of Piëtet. It was proposed or adopted by the late very ingenious Dr. Hutton. The facts, indeed, on which it was grounded may be satisfactorily explained from known principles. It was not consistent with strict metaphysics; and to bestow the property of being reflected on invisible light, was surely stretching the limits of probability. Yet was the hypothesis, in some degree, plausible and alluring. The little improvement of ascribing to it likewise refrangibility, by rendering the whole absurd, has dissolved the charm. What is the eye itself but a compound prism? And is not the expansion of the optic nerve adapted by its constitution to receive impressions through the diaphanous substance of the humours and coats, and to convey their appropriate sensations? But those are only sensations of light. Sensations of heat are confined to no particular class of nerves. Refrangibility is therefore correlative with vision, and "invisible refrangible light" seems a contradiction of terms. But all metaphysical considerations apart; if the image of the sun be not encircled with a broad lucid ring, on fixing our eye on that luminary, the sensation of heat ought not to reside in the corresponding spot on the retina, it should be more intensely felt over the surrounding space.

Let me conclude by recommending to your inquisitive readers two works of very superior merit. I mean the *Traité d'Optique* of Bouguer, and the *Photometria* of Lambert. The public has a right to expect that authors have previously studied the labours of their predecessors; but it would be charity to believe that some late writers had not consulted those excellent models, which might have prevented much unnecessary repetition, and corrected several gross errors.

I am, SIR,

Your most obedient servant,

JOHN LESLIE.

#### IV.

*Account of a Memoir of M. PROUST, on several interesting Points in Chemistry\*.*

THE first article relates to the *conversion of camphor into oil* by repeated distillations from a solar earth. This operation, he observes, was long since described by Neumann, and he appears to reproach Citizen Lagrange that he has not quoted this author in his Memoir

\* Translated from the Report made to the first Class of the Physical and Mathematical Sciences of the Institute of France by Cit. Vauquelin; Guyton and Vauquelin being commissioners. The report is inserted in the *Annales de Chimie*, XXXV. 32.

upon



upon Camphoric Acid\*, in which he has treated of the conversion of camphor into oil. Excepting this, nothing is said upon the subject by M. Proust but what is already universally known.

In the second article, which is more interesting than the first, he communicates some methods, more simple than those which he had previously pointed out, of *obtaining pure tannin*, the principle of which he has obtained from the excellent Memoir of Citizen Deyeux upon the Nut-gall.

The process consists in pouring a solution of carbonate of pot-ash into an infusion of galls. By the mixture of these two liquids, a yellowish-white precipitate is formed, in the form of a cogulum, which Ribaucourt took for an earth, and which it is only necessary to wash with a little cold water to obtain the pure tanning matter. But care must be taken not to wash it with too much, nor with warm water; for however little soluble this matter may be, it is nevertheless enough so, to disappear totally in a tolerably large quantity of water. Hence it follows, that in order to ensure complete success in the operation, the infusion of galls must not be too much diluted with water, since in that case there will be no precipitation.

It is no less essential that the alkali should be perfectly saturated with the carbonic acid, because an excess would favour the solution of a certain quantity of the tannin, which would be greater in proportion as the alkali was more caustic. Thus it is seen that the tannin is separated from its solution by an alkaline carbonate, not as might be supposed by saturating the gallic acid but by engaging its water of solution; for all the salts which have a certain degree of affinity with this liquid in the cold produce the same effect.

But however small the quantity of water that may be used, and whatever its temperature may be, some tannin always remains in solution; because it is soluble in a certain proportion even of the coldest water. The greatest part of this matter, which thus remains in solution, may be obtained by evaporating any portion of the liquid, and part of the tannin separates by cooling, which is more or less in proportion to the quantity of water evaporated. The tannin, when it has been thus separated from the other substances which accompanied it in the galls, has the appearance of a glutinous paste, of a yellowish grey colour, the parts of which adhere to each other in a considerable degree. It dries with difficulty; but when it is spread in thin layers upon earthen plates, and afterwards exposed to the heat of a stove, it first melts, then becomes dry, and at last acquires the appearance of a kind of yellow resin, with a vitreous fracture. This tannin, submitted to distillation, affords a saline liquid, in which the smell of ammoniac is distinguished, and which possesses the property of blackening solutions of the red oxide of iron; a property which must be attributed to a small quantity of tannin volatilized without alteration, and not to the gallic acid, because it does not afford a green colour with alkalis.

\* Philos. Journal, II. 97.

The oil which tannin affords by distillation is very small in quantity, and so thick, that it remains attached to the neck of the retort. Its coal is bulky, and forms  $\frac{1}{8}$  or 0.026 part of the distilled mass.

The liquid, from which the tannin has been separated by the carbonate of pot-ash, soon becomes green in the air, the oxygen of which it absorbs.

Sulphurated hydrogen destroys this green colour by uniting itself, in its turn, to the oxygen. Though the gallic acid renders tannin more soluble in water, as was remarked by Citizen Deyeux, nevertheless M. Proust does not believe that it is by combination with this acid that alkaline carbonates precipitate the tannin, since they also separate this matter from a simple solution in water, and because most of the very soluble neutral salts also precipitate it. It is, therefore, simply from a stronger affinity of these salts to water that these different substances separate the tannin from that fluid. The gallic acid possessing, however, the faculty of dissolving a certain quantity of this matter, and the carbonate of pot-ash possessing, at the same time, the double power of saturating this acid and of uniting itself closely to the water, it ought, under like circumstances of solubility, to leave less tannin in solution than the other salts.

M. Proust observes, that these facts, unimportant as they may seem to be, should put us upon our guard against the effects which take place between the solution of the tanning principle and animal fluids; indeed, since the saline materials, of which these liquids are never deprived, are able to precipitate the tanning principle, it ought not henceforth to be concluded from the appearance of the precipitate, at least not till the nature of the deposition has been examined, that animal fluids contain gelatin.

A piece of the muscle of an ox, or of raw hide, speedily discolours the infusion of gall-nut, and leaves nothing but gallic acid in the liquid. When these substances are saturated with the tanning principle, they easily dry without putrefaction; their fibres then separate and crumble into dust between the fingers, like wood which is worm-eaten. M. Proust very rationally considers the galling of wool, silk, and cotton, as a species of tanning; in which, by taking care not to carry the combination too far, the animal substance is made to acquire considerable hardness and resistance to the action of water, without losing too much of its flexibility, besides rendering it unfit to become the food of insects. He also demands whether the solution of tan would not answer the purposes of embalming better than any of the ingredients which have yet been made use of.

Citizen Dizé is the first person who observed that by pouring sulphuric or muriatic acid upon a rather strong decoction of galls, a considerable deposit is formed. M. Proust availed himself of this property to separate the tanning principle from the gallic acid with which it is mixed in the galls. It is, in fact, this substance which, by combining itself with the acids, becomes insoluble, and is precipitated from the water in the form of an adhesive pitch. When the precipitation is complete, the liquid which contains the gallic acid is decanted; the precipitate is washed with cold water; it is then dissolved in boiling water; the sulphuric acid is saturated with carbonate of pot-ash, and the tanning principle becomes



becomes precipitated as the pot-ash unites itself to the acid, and the sulphate of pot-ash, which has been formed, dissolves in the water. In order to obtain the whole of this substance, the liquid must be reduced by evaporation, and the deposit must be suffered to become cold, and washed with cold water. It is the pure tanning principle.

After describing the processes, which we have just related, for separating the principles of the nut-gall, namely, the tanning principle and the gallic acid, M. Proust next examines what takes place between these substances and iron in the making of ink. He remarks, first, that the reason why ink is not formed with a solution of iron at the minimum of oxidation is, because the tanning principle and the gallic acid have less affinity with the iron than the sulphuric acid has. 2dly, That when a solution of iron at the maximum of oxidation immediately produces a black colour with an infusion of galls, the reason is, that in this case the red oxide of iron has a greater attraction to the principles of the gall-nut than to the sulphuric acid. He proves this by dissolving the combination of iron with the tanning principle in sulphuric or muriatic acid, which gives a blueish transparent liquid, which would lose its colour if decomposition took place. He, therefore, considers ink as a solution of the tannate and the gallate of iron in sulphuric acid. 3dly, That when ink, spread upon paper, quickly becomes black, it is because it absorbs an additional quantity of oxygen, which renders it more insoluble in water. The combination of the oxide of iron with the gallic acid and with the tanning principle, which forms the basis of ink, contracts only a slight adherence with the acids, and separates itself from them by repose at the end of some time. In this case it is affected nearly in the same manner as metallic salts dissolved in a foreign acid; and for this reason it is that when a plate of iron is put into a solution of ink, this metal precipitates the black particles, absolutely in the same manner as iron separates the phosphate of iron from acids.

Alkalis, cautiously added to the solution of ink, precipitates its black parts, but an excess of alkali dissolves the atramentary combination, and produces a fluid of the colour of wine, more or less intense.

Gallic acid produces no effect on ink, because the oxide of iron is saturated. The same acid is not very active in shewing the presence of red oxide in a solution containing an excess of acid, because the combination is dissolved in proportion as it is formed. But by carefully saturating the excess of acid, the liquid is rendered black.

The combination of the gallic acid, and the tanning principle with iron, does not become changed into prussiate of iron by means of prussiate of pot-ash, even with the assistance of heat. The nitric acid dissolves the ink, and effects no change in its nature in the cold; but by heat it destroys it, and the yellow oxalate of iron is precipitated by the addition of ammoniac. Whenever iron filings and an infusion of galls are mixed together, a disengagement of hydrogen gas takes place; which proves, says M. Proust, that the oxidation of iron begins at the expense of the water, and that it is completed by the air, an event which also takes place with vegetable acids, which, not sufficiently attracting the oxide of iron oxidized to the minimum, cannot counterpoise the affinity of the oxygen which tends to ad-

vance it to the maximum; and this is the reason why the acetates of iron pass so rapidly to a red colour, that it is impossible to obtain them green in a solid state.

M. Proust prefers, both for durability and beauty, ink which is made by a solution of iron in an infusion of the gall-nut, to that which is prepared from sulphate of steel. His reasons are plausible. The infusion of acorns, concentrated and digested with iron filings, presented the same phenomena as the infusion of the gall-nut, and afforded M. Proust an ink, which, when mixed with a small quantity of vinegar and a sufficient quantity of gum, was inferior to none in its good qualities.

From the experiments of M. Proust, it follows, that there is nothing except the red oxide of iron which can make ink with the principles of the gall-nut; that the sulphates of iron, which are made use of for that purpose by ink-makers, containing always different quantities of red oxide, there as many different kinds of ink as there are recipes for making of it; but let these inks be only once spread upon paper, and the air reduces them all to nearly the same state.

Every one is acquainted with the property possessed by inks which are too much diluted; of becoming black after they are spread upon paper by the pen; but this phenomenon may be produced in a much more striking manner. It is only necessary to pass some sulphurated hydrogen gas into the ink, and the black colour will instantly disappear; but afterwards, in writing, it is very pleasant to observe this clear liquid become very speedily black.

Such are the principal facts which have been communicated by M. Proust respecting the combination of iron with the principles of the gall-nut; namely, the tanning principle and the gallic acid. They serve to illustrate the theory and the practice of the arts of dyeing and of ink making. He has promised the Institute to pursue his researches on this subject still further.

In another article, M. Proust proposes a *method of obtaining sulphuric acid from the residues of the distillation of sulphuric ether*. This simple process consists in diluting the residue with two parts of water, filtering through a cloth to separate the coaly substance from it, and subjecting the liquid to distillation in a glass retort. When the acid has acquired 1.84 of specific gravity, from 4 to 6 grammes (parts) of saltpetre to each demikilogram (five hundred parts) are added; and the distillation is continued till the fluid is perfectly white, and weighs from 1.86 to 1.87, water being 1.00.

M. Proust prefers an iron frame to a sand bath to place the retort in for these distillations; and he gives very good reasons for the preference. He computes that the profit of purifying the residues of ether in this manner will be considerable; since it affords between a fourth and a third part of sulphuric acid concentrated to 1.87 specific gravity.

The sulphuric acid, separated from the butuminous matter before it is rectified, has been subjected to some experiments by M. Proust. He observed, that alkalis, as well as the prussiates, did not precipitate any thing. It abundantly decomposes hydro-sulphurated

water



water by virtue of the sulphureous acid it contains: oxygenated muriatic acid discolours this brown liquid; which, in his opinion, proves that this colouring material resembles the juices of vegetables. The coaly substance which is separated during the formation of sulphuric ether has also been made an object of M. Proust's researches: when dried it resembles lamp black (*noir de fumée*) as well in its black colour, its brittleness, and its shining colour. If exposed to heat, it emits a smell of sulphureous acid mixed with that burning lac. Boiling water dissolves 0.24 parts of this substance, alcohol as many as 0.44, and this solution is not rendered turbid by water. These two solutions are proper for the formation of brown dyes by means of the muriate of tin.

The solution of the coaly substance of the ether in alcohol affords, by evaporation, a black friable and nearly insipid powder. 5.31 grammes (100 Fr. grains) of this substance distilled produced an aromatic water, light and empyreumatic oils, vegetable acid mixed with a little sulphuric acid, and carbonated oily hydrogen gas, mixed with about a third of carbonic acid. The coal which remained in the distilling vessel weighed 0.64 grammes (12 Fr. grains); it left, after combustion, some ashes, in which analysis discovered the presence of lime and magnesia. M. Proust, indeed, suspects that there is a small quantity of siliceous earth and alumine in it. Here, then, observes this skilful chemist, we have earthy ashes, which accompany carbon even into alcohol.

M. Proust concludes that the principles of the alcohol in forming ether by the action of the sulphuric acid become united in other orders, and produce a substance analogous to the juices of vegetables, since, during the summer, it is covered with mouldiness like a vegetable decoction.

M. Proust also discovered that, during the distillation of phosphorus, a combination of this substance with charcoal is constantly formed, and that it is this combination which remains in the chamois leather after phosphorus has been passed through it for its purification, after the manner of Pelletier. It is of a red colour, and does not melt like pure phosphorus. If it be distilled by a gentle heat, a portion of phosphorus is separated, which exceeded the point of saturation; but the true combination is not decomposed, unless the degree of heat be considerably augmented. When the vessels become cool, a powder remains of lively orange red, light, flocculent, and very homogeneous through its whole mass. M. Proust considers this product as an intimate union of carbon and phosphorus. The following are some of its properties which he describes. If, whilst it is still in the retort, the temperature be sufficiently raised to ignite the bottom, a fresh quantity of phosphorus is sublimed, and the residue is then mere coal. It burns rapidly when placed upon a plate of hot metal; but the coal, by imbibing phosphoric acid, eludes the combustion. By exposure to the air, it soon loses this disposition to take fire, and may then be preserved without any danger. It is without smell or taste. It is this combination, according to M. Proust, which forms the red residues which remain without alteration in the apparatus in which phosphorus is prepared. Its properties serve to explain the origin of a black powder which some chemists have observed in phosphorus. There cannot be a

stronger proof, continues M. Proust, that the phosphuret of carbon is a combination formed by virtue of the laws of affinity, than its unchangeableness by the solution of caustic pot-ash, even with the assistance of heat. Time did not permit the author to determine the proportions of the elements of the phosphuret of carbon. He promises a plentiful harvest of new facts to those who may be disposed to pursue this subject, and follow its combinations with metals and other combustible bodies.

On this occasion M. Proust corrects or recalls an opinion which was given in the *Journal des Mines*, respecting the smell of hydrogen gas which becomes disengaged during the solution of certain kinds of cast iron and steel in acids. He conceives that it is owing to the presence of an essential oil rather than to that of phosphorus, as had been suspected. He gives the following observations in support of his present notion: 1°. The neck of the matras, retorts, and receivers, in which the inflammable gas is prepared, are greased with little drops of this oil. 2°. If 15 ounces of black crude iron be dissolved in sulphuric or muriatic acid, drops of oil are observed floating on the water of the receiver. 3°. If the washed carburets, which are obtained from crude iron, be gently distilled, water comes over along with an oil of the same smell; and alcohol, in which such crude iron has been macerated (in powder) becomes white on the addition of water. M. Proust thinks that if the inflammable gas, which is obtained from crude iron, were carefully examined, it would be found to hold oil in solution, and that it is this substance which gives it its fetid smell. If the fact be accurately so, of which we have no doubt, it is, as M. Proust observes, a grand step towards the explanation of the phenomena of vegetation, and the understanding the transition of mineral substances into vegetable products.

M. Proust thence passes to the examination of the native of the iron of Peru. This species of iron, which has been noticed by Rubin de Celis, is of a greyish white colour. It bears a rather strong resemblance to certain native silvers, for which it was for some time mistaken. It possesses a considerable degree of ductility, does not rust like common iron, and has nearly the same habitude under the file.

5.31 grammes (100 grains) of this iron, dissolved in sulphuric acid, only afforded 3520 centimetres (176 Fr. inches) of hydrogen gas; whilst the same quantity of common iron wire gave 200 inches. At the commencement of the solution, a small quantity of carbonate of iron was separated, which disappeared towards the end.

The colour of the solution, which was much greener than usual, led M. Proust to suspect that there was copper in this iron; but sulphurated hydrogen not having precipitated any thing, and the liquid having preserved the same colour as before, it appeared to him that no other substance but nickel could produce these effects. In order to have a certain proof of this, he oxidized the iron strongly by means of nitric acid, and afterwards precipitated it by the gradual addition of pot-ash. The iron being thus separated, the green colour of the liquid assumed a greater degree of intensity, and by completing the precipitation, after filtering, he obtained a substance which, with sulphuric acid, yielded

sulphate



sulphate of nickel. This method of analysis, which M. Proust has indicated, in combination with the use of sulphurated hydrogen, was found by him to be equally successful in the complete separation of nickel from iron, arsenic, and copper, which are almost always met with in the ores of this metal.

The native iron of Peru is therefore, according to the experiments made by M. Proust, an alloy of iron and nickel; a new discovery of the most interesting nature. The presence of nickel in this alloy, observes the author, appears to announce that it is the product of art; but when it is considered that there exists a mass of more than 1363 myriagrams (300 quintals) in a plain of more than 100 leagues in circumference, where there is neither mountain nor water, nor scarcely a stone is to be found, the difficulty of the problem still remains in all its force. Lastly, adds M. Proust, if the power of uniting these metals in suitable proportions can be obtained by metallurgists, they will have obtained an alloy which will possess many advantages over other iron, and more particularly that of not being liable to rust.

M. Proust's memoir is concluded with some detached facts, as well on animal as on mineral substances. 1st. A myriagram (20 Fr. pounds) of beef, of which 25 hectograms (5 pounds) were bone, produced only one pound of common brown extract, which was elastic, and of a taste similar to concentrated or very strong soup. It would be impossible to convert this extract into portable pastels without the addition of a rather large quantity of the jelly of bones. Whenever beef is boiled in silver, this metal becomes black, as is the case when it is used for the boiling of whey, fresh urine, &c. 76 hectograms (15 Fr. pounds) of meat were reduced, by boiling, to ten pounds; but the bones lost none of their weight: whence it follows, that in order to extract as much nutriment as is possible from these substances they must be broken to pieces.

The fresh decoction of flesh is acid, and reddens the tincture of turnsole. The acid which it contains appears to be the phosphoric; for lime water and ammoniac form a rather abundant precipitate with it. Alcohol dissolves a portion of the extract of flesh, and this part being extremely salt, M. Proust suspected the presence of muriate of ammoniac; but lime in powder not having separated any ammoniac, he mixed a solution of platina with it, which immediately furnished him with muriate of platina and pot-ash. The decoction, therefore, contains muriate of pot-ash in abundance. M. Proust had not time to examine the other salts contained in the decoction, nor the substance soluble in alcohol; and he, with reason, complains that a substance so necessary for the nourishment of mankind, has not been made the object of serious chemical research.

He made some trials upon the pyrites of the Incas, to discover if it did not contain gold or some other substance which might serve to explain the cause of the pale colour which distinguishes it. He only obtained, as the residue of his solution, a black powder mixed with sand. This powder was coal, which caused a strong detonation with nitre.

It has been thought, and it is still generally supposed, that the black dust which is deposited during the solution of zinc in acids is carburet of iron; but M. Proust found that it was  
a mixture

a mixture of arsenic, copper, and lead which the disoxidating action of the zinc precipitates in the metallic state. This also happens to these metals when a solution is made of tin with which they are alloyed. Arsenical lead, such as is made use of at the mines of Limares for the making small shot, deposit on the immersion of a sheet of lead, part of the metallic arsenic they contained. These are not the only metals which adulterate the purity of the zinc. Iron and manganese are sometimes found in great quantities: whence it is not surprising that the art of watch-making has so much reason to complain of the bad qualities of brads made in the common way. The following facts will shew the enormous difference between purified zinc, and that of commerce. To purify zinc, Mr. Proust proposes distilling it in a stone retort, the neck of which must be inclined at least 45 degrees, that the metal may flow with greater ease as soon as it becomes volatilized. A mixture of sand, and of the oxides of iron, lead, copper, and zinc remain in the retort, the oxidation of which is attributed by M. Proust to the porosity of the vessels.

The zinc thus purified does not differ either in colour or in weight from the zinc of commerce: 55 decigrams (100 Fr. grains) of the latter yield by solution in sulphuric acid, in less than hour, 3440 centimetres (from 172 to 174 Fr. inches) of hydrogen gas; whereas more than 8 days are requisite to obtain the same result from distilled zinc. This is the difference which M. Proust observed between the two metals. The black matter which separates from the zinc, during its solution in sulphuric acid, is soluble in nitric acid; and its solution on the addition of hydro-sulphurated water affords arsenic; and if that arsenic had been mixed with copper or lead, the same re-agent would have equally discovered it. For by adding it gradually to the solution of those three metals, the copper is seen to precipitate itself first of a brown colour the lead, the next of a black colour, and the arsenic last of a yellow colour; and by a careful management they may be separated by filtration.

M. Proust justly considers the purification of zinc by sulphur as an absolute deception.

55 decigrams (100 Fr. grains) of zinc, dissolved in nitric acid, leave, after the decomposition of the nitre by fire, 69 decigrams (from 125 to 126 Fr. grains) of oxide of a light yellow colour. A solution of the same quantity of zinc, decomposed by carbonate of pot-ash, furnished 99 decigrams (180 Fr. grains) of carbonate of zinc, which also left 125 grains of oxide after calcination.

The solution of this metal by sulphuric acid gave the same results; which proves that zinc, in all cases, combines itself with a specific quantity of oxygen. M. Proust observes how singular it is that copper and zinc, which attract the oxygen with such different degrees of strength, should, nevertheless, in their combination with acids, absorb exactly the same proportion of this principle.

M. Proust, in the next place, proposes the following formula to separate zinc from copper. Suppose the same solvent to contain lead, copper, and zinc. Sulphate of pot-ash will precipitate the lead, if the (nitric) solution does not contain too much acid. Hepatic water separates the copper a long time before the zinc. The filtered liquid may be examined by  
fulphurated



fulphurated hydrogen. When it no longer becomes coloured, a greater quantity of hydro-fulphurated water must be added to it, when the zinc will in its turn fall down of a light yellow colour. If there had been iron, cobalt, nickel, or manganese in the solution none of them would have been precipitated by this re-agent.

From the preceding experiments on zinc, M. Proust draws the following conclusions. 1st. In whatever acid this metal be dissolved, it constantly absorbs the same quantity of oxygen. 2d. If it contain metals capable of superoxydation, they pass to this state when the solution has been effected either by nitric or oxygenated muriatic acid. 3. In a muriatic or sulphuric solution this metal is as much oxidized as it can be, but iron on the contrary is only at its minimum; and this is the reason why this solution does not become coloured with gallic acid, unless it has been exposed to the air. 4th. To demonstrate the presence of iron some drops of oxygenated muriatic acid must be added, or it may be boiled with a small quantity of nitrous acid. 5. The carbonate, which is very white whilst under water, becomes yellow as soon as it is brought into the air, because the iron passes quickly to the maximum of oxidation. 6th. Sulphate of zinc, which has had time to absorb in the atmosphere as much oxygen as is necessary to elevate the iron to its maximum, immediately affords yellow carbonate of zinc. 7. Zinc, which is purified by distillation, or by the following humid process, is alone proper to afford the true zinc-white for painting.

Repeated crystallization of the sulphate of zinc, and immersion of plates of this metal into its solution appeared to M. Proust, to be very insufficient means to separate the foreign metals. To accomplish this object, 30.57 grams (one Fr. ounce) of nitric acid are put into about 2 pounds of the saturated solution of sulphate of zinc, and the mixture slightly boiled. Pot-ash is then mixed with it to saturate the excess of acid, and to precipitate nearly 15.28 grams (3 or 4 gros) of the matter; this mixture is boiled again, and the precipitate is soon observed to pass from white to yellow. If after some minutes of ebullition some white particles are remarked amongst the yellow deposit, it is a certain sign that not an atom of iron remains in the solution of zinc. But if by this method the iron has been entirely separated from the zinc, manganese may yet be present, if any portion existed in the ore; and M. Proust has frequently met with it.

To clear it of this new oxide the sulphate of zinc is dissolved in boiling water, and precipitated by carbonate of pot-ash, so as still to leave a small quantity of oxide of zinc in solution. The deposit is kept several days in the liquor, in order that the oxide of manganese, which has been precipitated, being more strongly attracted by the acid than the oxide of zinc, may precipitate that portion of the latter which had been left in the fluid, and become dissolved in its place. Then the sulphate of zinc affords an oxide of the most perfect whiteness which may be employed, very advantageously, for painting.

These are the facts contained in M. Proust's Memoir. They are numerous and interesting. Some of them are new to us; the most part, though already known, are presented under new relations and rendered applicable to the arts. The experiments, by the help of which he has discovered these facts, are ingenious. His explanations, though concise, appear

appear to us to be clear, and deduced immediately from the results of the experiments. They may therefore tend to the advancement of philosophical chemistry, and to the perfection of the arts and manufactures. We, consequently, recommend to the class (of the national institute) to order them to be printed in the volumes which it proposes to publish. We, cannot, however, agree with M. Proust in some points. 1st. That nickel, cobalt, and manganese are not precipitated by fulphurated hydrogen. 2. That the black dust, which separates during the solution of zinc, does not contain carburet of iron. 3d. That zinc is separated from arsenic by distillation. The contrary is so well known by chemists, that it requires no proof.

## V.

*Experiments on the solar and on the terrestrial Rays that occasion Heat ; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same or different. By WILLIAM HERSCHEL, LL. D. F. R. S \**

THE word heat is most commonly used to denote a certain well known sensation. It is also employed to signify the cause of that sensation, as well by the vulgar as by men of science : some of whom have added certain distinctive terms, such as latent, absolute, sensible, radiant, while others have treated of the matter of heat, and caloric. Dr. Herschel has not thought fit to adopt any of these terms in his Memoir, but has chosen to treat of *the rays that occasion heat* ; not meaning either to state that these rays are heat, or the manner in which they produce it. In his prefatory observations he also in effect remarks that his present research is confined to the agency of heat in its state of radiance ; without entering into any considerations respecting the general nature of heat itself, whether it be matter or modification, or of radiance, whether it be the projection of particles, the undulation of a fluid, or any other habitude or thing.

His subject is reduced to three general heads : the first relates to the heat of luminous bodies in general as it comes directly from the sun, or from the terrestrial flames of torches, candles, lamps, blue lights, &c.—the second includes the heat of coloured radiants as from the sun, when its rays are separated by the prism, or from culinary fires openly exposed ;—and the third division relates to heat obtained from radiants, where neither light nor colour in the rays can be perceived. This is to be had as before shewn †, directly from the sun, by means of a prism applied to its rays, and also from fires inclosed in stoves, or from red-hot iron, cooled, till it can no longer be seen in the dark.

\* Abridged from the Phil. Trans. for 1800, page 293.

† Philos. Journal IV. 320.



Besides the foregoing general arrangement, the author gives a previous comparative view of facts relating to light and heat as follows:

1. LIGHT, both solar and terrestrial, is a sensation occasioned by rays emanating from luminous bodies which have a power of illuminating objects; and according to circumstances of making them appear of various colours. 2. These rays are subject to the laws of reflection. 3. And also those of refraction. 4. They are of different refrangibility. 5. Are liable to be stopped in certain proportions when transmitted through diaphanous bodies. 6. And to be scattered on rough surfaces. 7. And lastly, they have hitherto been supposed to have a power of heating bodies, but this remains to be examined.

1. HEAT, on the other hand as well terrestrial as solar, is a sensation occasioned by rays emanating from candent substances which have a power of heating bodies. These rays have the same affections as have been enumerated in the 2, 3, 4, 5, and 6th divisions of the last paragraph, and 7. They may be supposed, when in a certain state of energy, to have a power of illuminating objects; but this remains to be examined.

The paper before us contains the first part of the experimental investigation of the subject under the three first heads. The three next, and a discussion which will be brought on by the seventh article, are reserved for the second part.

*Experiment 1. Reflection of the heat of the sun.* The sun's rays were received in a ten foot Newtonian telescope, with a camera eye piece, with no eye glass. By proper adjustment the focus was made to fall on a small thermometer, which was raised 58 degrees. The rays, therefore, whether of light or not, did occasion heat after three regular reflections.

*Experiment 2. Reflection of the heat of a Candle.* At the distance of 29 inches from a candle a small steel mirror, 3, 4 inches diameter, and about 2, 75 inches, focal length, was placed. Two thermometers were disposed, one in the secondary focus, and the other very near it, but out of the course of the reflected light. In five minutes that in the focus rose  $3\frac{1}{2}$  degrees, which it lost again in six minutes when the candle was covered, and again recovered it in five minutes when the rays were suffered to fall on the mirror. The other thermometer remained stationary throughout.

*Experiment 3. Reflection of the heat that accompanies the solar prismatic colours.* The solar spectrum given by a prism was admitted through pasteboard (which limited the visible colours) upon the steel mirror. The thermometer in the focus was raised  $35^{\circ}$  in two minutes.

*Experiment 4. Reflection of the heat of a red hot poker.* The mirror was placed at the distance of 12 inches from a red hot poker, and the thermometer placed in its secondary focus. A small pasteboard screen was placed to guard the thermometer from the direct heat. In one minute and a half it rose  $38\frac{1}{2}^{\circ}$ , and fell  $28^{\circ}$  in the next minute and a half, when the mirror was covered.

*Experiment 5. Reflection of the heat of a coal fire by a plain mirror.* A small speculum (Pl. XVI. fig. 1.), such as the Doctor uses with his seven feet reflectors was placed upon a stand, so as to make an angle of  $45^{\circ}$  with the front of it, and consequently with the bars

of a grate opposite which it was placed. At a distance of  $3\frac{1}{2}$  inches from the speculum, on the reflecting side of it, was placed a thermometer, and another close by it out of the course of the reflected rays. The whole was guarded in front against the influence of the fire by an oaken board,  $1\frac{1}{2}$  inch thick, in which was a circular opening of  $1\frac{1}{4}$  inch diameter, opposite the plain mirror, to permit the fire to shine upon it. There was also a wooden partition between the mirror and the thermometers in which was a hole that allowed the rays to fall on the first mentioned thermometer, but prevented their access to the latter. When this apparatus was exposed to the fire the first thermometer rose 7 degrees in five minutes, while the other indicated a change in the temperature of the place, amounting to no more than half a degree.

*Experiment 6. Reflection of fire-heat by a prism.* When the last described experiment was repeated by substituting instead of the mirror a prism whose angles were  $90^\circ$ ;  $45^\circ$ ; and  $45^\circ$ , so placed that the radiation from the fire should pass through one side, and make its incidence on the hypotenuse within the prism, the rise was  $4\frac{1}{2}$  degrees in 11 minutes; but the temperature of the place, as shewn by the other thermometer, having been raised  $1\frac{3}{4}$  degrees in the same time, the effect of the reflection was only  $2\frac{1}{4}$  degrees.

*Experiment 7. Reflection of invisible solar heat.* On a board of about 4 feet, 6 inches long, a small plain mirror was fixed at one end, and at the other, two thermometers; as shewn in fig. 2. Pl. XVI. where for the convenience of insertion the board is represented as broken. The distance of one thermometer from the face of the mirror was 3 feet  $9\frac{1}{4}$  inches; and the other thermometer was put at the side of it facing the same way, but out of the course of the rays, that were to be reflected from the mirror. The colours of the prism were thrown on a sheet of paper, having parallel lines drawn upon it at half an inch from each other. The mirror was stationed upon the paper; and was adjusted in such a manner as to present its polished surface in angle of  $45^\circ$ , to the incident coloured rays, by which means they would be reflected towards the ball of the first mentioned thermometer. In this arrangement the whole apparatus might be withdrawn from the colours to any required distance, by attending to the last visible red colour, as it shewed itself on the lines of the paper. When the thermometers were properly settled to the temperature of their situation, during which time the mirror had been covered, the apparatus was drawn gently away from the colours so far as to cause the mirror, which was now open, to receive only the invisible rays of heat which lie beyond the confines of the red. The result was, that in ten minutes the first thermometer received four degrees of heat reflected to it in the strictest optical manner, and the great regularity with which these invisible rays obeyed the law of reflection was such that the very sensible thermometer, which was chosen as a standard, and was within an inch of the other, remained all the time without the least indication of any change. The mirror was then taken away, and in ten minutes the measuring thermometer again lost the heat it had required. *This therefore, as the Doctor observes, is a most decisive experiment in proof of the existence of invisible rays, of their being subject to the laws of reflection, and of their power of occasioning heat.*

*Experiment*



*Experiment 8. Reflection and condensation of the invisible solar rays.* The small steel mirror before described was by a suitable apparatus placed to receive the rays of the prismatic solar spectrum perpendicularly to its diameter or subtense. Half the mirror was covered by a semicircular pasteboard with lines drawn thereon parallel to the diameter, by means of which the last visible red might be made to fall at any required distance from the uncovered portion of the speculum. The thermometer was placed in the focus, and the whole surface of the mirror kept covered till the instrument became stationary at the temperature of the place. At this period the apparatus was moved till the last visible red fell on the graduated pasteboard at the distance of one-tenth of an inch from its diametrical edge, when the other part of the reflecting surface being uncovered for the admission of the invisible rays, the thermometer in one minute was raised 19 degrees. The mirror was then again covered, and in 3 minutes the thermometer fell 16 degrees.—Again, the mirror was opened, and in 2 minutes the thermometer rose 24 degrees. And on covering the mirror once more, in one minute the thermometer fell 19 degrees.

By this alternate rising and falling our author observes, that there are three points clearly ascertained. First, that there are invisible rays of the sun. Secondly, that these rays are not only reflexible in the manner proved in the foregoing experiment, but that by the strict laws of reflection they are capable of being condensed. And thirdly, that by this condensation their heat is proportionally increased.

*Experiment 9. Reflection of invisible culinary heat.* The steel mirror was fixed upon a small board, and before it was a wooden screen, half an inch thick, which was of an height to intercept the rays which might else have fallen on the lower half of the mirror. Behind this screen were placed two thermometers; one of which was in the axis of the mirror, and the other, which was near it, was screened on the side towards the mirror by a small slip of pasteboard tied to the scale. This apparatus being duly placed opposite a close stove, well heated, the invisible rays reflected from the mirror raised the thermometer in its axis 39 degrees in one minute, while the other, which had exactly the same exposure to the stove, but was defended from the reflected rays of the mirror, rose only one degree.

*Experiment 10. Reflection of the invisible rays of heat of a poker cooled from being red-hot till it could no longer be seen in a dark place.* A poker of a proper black heat was placed at the distance of 12 inches from the steel mirror, and the effect of its condensed rays was received upon the thermometer placed in the focus. The mirror was uncovered and covered alternately, one minute at a time, for six minutes, and the alternations were, rise 7°; fall 7°; rise 3°; fall 5°; rise 2½°; fall 6½°.

Dr. Herschell concludes his narration of facts concerning reflected heat with the following remarks:

“ From these experiments it is now sufficiently evident, that in every supposed case of solar and terrestrial heat, we have traced out rays that are subject to the regular laws of reflection, and are invested with a power of heating bodies; and this independent of light. For though, in four cases out of six, we had illuminating as well as heating rays,

“ it is be noticed that our proof goes only to the power of occasioning heat, which has been  
 “ strictly ascertained by the thermometer. If it should be said that the power of illuminating  
 “ objects, of these same rays, is as strictly proved by the same experiments, I must remark  
 “ that from the cases of invisible rays brought forward in the four last experiments, it is  
 “ evident that the conclusion that rays must have illuminating power, because they have a  
 “ power of occasioning heat, is erroneous; and, as this must be admitted, we have a right  
 “ to ask for some proof of the assertion, that rays which occasion heat can ever become vi-  
 “ sible. But, as we shall have an opportunity to say more of this hereafter, I proceed now  
 “ to investigate the refraction of heat-making rays.”

*Experiment 11. Refraction of solar heat.* The sun's light was received in a new ten feet Newtonian telescope; the mirror of which has 24 inches in diameter of polished surface. The rays were made to pass through a day-piece with four lenses, and thence to the ball of the thermometer placed in their focus. As soon as the rays were brought to the thermometer it rose almost instantly through  $70^{\circ}$ , when the telescope was turned away to avoid the danger of cracking the glasses. Here the rays which occasioned this sudden augmentation of heat had undergone eight successive refractions, so that their being subject to its laws cannot be doubted.

*Experiment 12. Refraction of the heat of a candle.* A lens of about 1.4 inch focus, and 1.1 diameter, mounted on a small support was placed at the distance of 2.8 inches from a candle, and a thermometer was very carefully placed in the secondary focus of the candle behind the lens. Not far from the lens towards the candle was a pasteboard screen with an aperture of nearly the same size as the lens; and the lens itself was supported on an eccentric pivot, so that it could be turned away from its place and restored at pleasure. This arrangement being made, the thermometer was for a few moments exposed to the rays of the candle till it had assumed the temperature of its situation. Then the lens was turned on its pivot, so as to intercept the direct rays which passed through the opening in the pasteboard screen, and to refract them to the focus in which the thermometer was placed. In three minutes the thermometer received  $2\frac{1}{2}$  degrees of heat by the refraction of the lens, which it again lost in the same time when the lens was turned away, and in three minutes more the same increase of heat was produced by replacing the lens in its situation. A greater effect could be produced by a different arrangement of the distances. Thus when the lens was placed at  $3\frac{1}{2}$  inches distance from a wax candle, the thermometer in the secondary focus was raised from 5 to 8 degrees, according to the burning of the candle and the accuracy of the adjustment.

*Experiment 13. Refraction of the heat that accompanies the coloured part of the prismatic spectrum.* The coloured part of the prismatic solar spectrum being admitted through a perforated screen of pasteboard, upon a burning glass, raised the temperature of a thermometer  $112$  degrees in one minute.

*Experiment*



*Experiment 14. Refraction of the heat of a chimney fire.* The same burning glass which is nearly 9 inches in diameter was placed before the clear fire of a large grate, at the distance of 3 feet from the bars, and a thermometer was placed in the secondary focus. Another thermometer was also placed behind a wooden screen at the same distance from the fire. The situation of the several parts of the apparatus being thus adjusted the thermometers were taken away to be cooled, and then restored to their places. The standard thermometer in the course of the first seven minutes acquired a heat of  $3\frac{3}{4}$  degrees, after which it remained steady for the subsequent 32 minutes duration of the experiment. The other thermometer was treated in the same manner as that which received the refracted heat from a candle in experiment 12; that is to say, the lens was alternately placed and removed, and the variations were—rise in the first 9 minutes,  $9\frac{1}{4}$  degrees; in the next 5 minutes, before the uncondensed rays of the fire, fall  $2\frac{1}{4}$  degrees;—in the next 10 minutes rise  $1\frac{3}{4}$  degrees; in the next  $5\frac{1}{2}$  minutes, fall 3 degrees; and in the next  $4\frac{1}{2}$  minutes, rise  $1\frac{1}{2}$  degrees.—So that in the course of 35 minutes, the thermometer was raised and depressed 5 times by rays which came from the culinary fire, and were subject to laws of refraction, not sensibly different from those which affect light.

*Experiment 15. Refraction of the heat of red hot iron.* A lump of iron was forged into a cylinder  $2\frac{1}{2}$  inches in diameter, and  $2\frac{1}{2}$  inches long. This being made red hot, was stuck upon an iron handle fixed on a stand, so as to present one of its circular faces to a lens of the dimensions used in the 12th experiment, at the distance of 2,8 inches. Before the lens, at some distance, was placed a screen of wood, with a hole of an inch diameter in it, to limit the object, and to keep the heat from the thermometers. One of these was placed in the secondary focus of the lens, and the other, within  $\frac{3}{8}$  of an inch of that focus, and at the same distance from the lens, so that if there was any difference in the exposure, it was in favour of the last thermometer, which stood opposite a thinner part of the lens. During the experiment, the thermometers were alternately screened from the effect of the lens, and exposed to it for the same length of time, and the effects were, that both underwent variations of increased and diminished temperature; but that which was in the focus gained 6 degrees in the first two minutes, whilst that near the focus gained only 4; and the first lost 3 degrees in the second two minutes during the time the lens was screened, whilst the second lost only 2; and so in three other alternations the alternations of temperature in the focus were  $2^{\circ}$ ,  $2\frac{3}{4}^{\circ}$ , and  $1\frac{1}{4}^{\circ}$ , whilst the corresponding changes out of the focus were  $1^{\circ}$ ,  $1\frac{1}{4}^{\circ}$ , and  $0\frac{1}{2}^{\circ}$ . To remove all doubt on the subject, the thermometers were left in their respective situations, and a plain glass substituted instead of the lens. In these circumstances the last related process of alternate covering and uncovering the glass was repeated, and both thermometers were found to be affected alike.

*Experiment 16. Refraction of fire heat by an instrument resembling a telescope.* This instrument consisted of a concave mirror, which received the heat from the fire, a plain mirror which reflected it to one side, where there was a lens at the usual eye aperture. A thermometer was placed in the focus of this lens, and another near the focus, and the same method.

method of comparative examination was adopted as in the last experiment. The result also was similar, for which reason I shall consult brevity so far as to avoid repeating the particulars in detail.

*Experiment 17. Refraction of the invisible rays of solar heat.* One half of the before mentioned burning lens was covered, and the prismatic spectrum thrown upon the cover, so that the last visible red colour falling one-tenth of an inch from the margin of the pasteboard, the invisible rays beyond the spectrum were suffered to fall upon the lens. In the focus of the red rays, or a very little beyond it, was placed the ball of one of the thermometers, and the other thermometer as near to it as convenient. The thermometer in the focus was raised  $45^{\circ}$  in one minute, whilst the other thermometer underwent no change.

As a little of the red colour was discernible on the ball of the thermometer, the Doctor was induced to try if the invisible rays could not be rendered perceptible to the sight by condensation; and with this intention the next experiment was made.

*Experiment 18. Trial to render the invisible rays of the sun visible by condensation.* The last described experiment was repeated by withdrawing the lens till the extreme visible colour was  $\frac{2}{10}$  from the margin of the semicircular pasteboard cover. In these circumstances there was no longer any tinge of colour or vestige of light to be seen on the ball of the thermometer; but, nevertheless, the effect of the invisible rays was such, that the thermometer in the focus received  $21^{\circ}$  of heat, whilst the other near the focus remained stationary. The colour seen in the 17th experiment must therefore, as the Doctor observes, have arisen from the imperfect refraction of a lens of so great a diameter; the difficulty of ascertaining the termination of a prismatic spectrum in a room not perfectly dark, and the gradual diffusion of rays of the same kind over a considerable space in consequence of the breadth of the prism.

*Experiment 19. Refraction of invisible culinary heat.* This experiment is attended with some difficulties, on account of the feeble stock of heat in a red hot lump of iron, or other similar thing, and the speed with which it is carried off. The Doctor was prevented from adopting some contrivance to keep up this heat from the consideration that the alternate rising and falling of a thermometer in the focus of a lens, successively covered and uncovered, must be ascribed to the refraction of heat. The lens of 1.4 focus was placed before the cylinder of iron of experiment 15, made very red hot, and the thermometer placed in its focus was for each successive two minutes first exposed to the heat, and then screened from it by a small piece of pasteboard. The iron was at the beginning very red hot. The changes, after the iron had ceased to emit any light perceptible in a darkened room, were eight in number, and regular, amounting to one degree at the beginning, and at the end the effect of the condensed rays exceeded but half a degree the loss of those which were stopped by the lens.

*Experiment 20. Confirmation of the last experiment.* The 19th experiment was repeated with an assistant thermometer. The principal thermometer being placed in the focus, as



before the assistant thermometer was placed  $\frac{3}{4}$  of an inch distant on one side, and, consequently, was exposed to the direct action of the heated cylinder. The screen was never advanced before this last. During eight minutes, the thermometer in the focus being uncovered for the first minute, covered for the second, uncovered for the third, and so forth, came to its maximum by alternate greater elevations and smaller depressions, and afterwards lost its heat by greater depressions and smaller elevations. But the other thermometer being out of the reach of refraction, acquired its maximum of heat gradually, in consequence of an uniform exposure to the heated cylinder, and afterwards began to decline.

After the first eight minutes, the assistant thermometer was removed to one side of the focus, so as to participate of the alternate screening, and also to receive a small share of one side of the invisible heat image, which must be formed in the focus of the lens. Here, in confirmation of the reasoning flowing from the general facts and doctrines, the assistant thermometer should be affected by alternate risings and fallings, but less considerable than those in the focus. The changes of the focal thermometer were for six alternations in ten minutes —  $\frac{3}{4} + \frac{1}{2} - 1\frac{1}{4} + \frac{3}{4} - 1 + 1$ , and those of the assistant thermometer were —  $\frac{1}{4} + \frac{1}{4} - \frac{3}{4} + \frac{1}{4} - \frac{1}{2} + \frac{3}{4}$ , all which, as our author observes, so clearly confirm the effect of the refraction of the lens, that it must be evident that there are rays issuing from hot iron, which, though in a state of total invisibility, have a power of occasioning heat, and obey certain laws of refraction, very nearly the same with those that affect light. From the whole retrospect of his paper, he adds, that it will be easy to perceive that he has made a good proof, in this division of his memoir, of the three first of his propositions.

## VI.

*On the Arragonite of Werner. By Cit. Haüy\*.*

THE mineral which is the object of this Memoir presents itself in the form of hexahedral crystals of a dirty, uneven, violet colour; some of them have their bases smooth, but commonly tarnished, whilst others instead of bases have a multitude of angular projections. Their specific gravity is 2,9455; their refraction double in a very striking degree. They easily scratch carbonated lime, and appear to possess the same hardness as fluated lime; their powder thrown upon burning coals gives a violet coloured phosphorescence. They are entirely soluble with a lively effervescence in nitric acid. They are found in Spain between the kingdoms of Arragon and Valencia, which is the reason of their being called Arragonites, by Werner. There are some also to be met with near the Pyrenean Mountains.

Their phosphorescence, together with their colour, induced Born to suspect the presence of phosphate of lime; but Klaproth found only carbonate of lime.

\* Bulletin de la Soc. Philomath. No. 33, tome 2.

Haüy having endeavoured to divide some of these crystals mechanically, observed that they had natural planes of junction situated parallel to their prisms which indicated a striking difference between their structure and that of carbonated lime. He has also remarked, that there is a difference of several degrees in the mutual incidences of the faces of the prism, and that this inequality varies in different crystals. This variation of the angle would be inexplicable on the hypothesis of a simple crystal where there should be unity of structure; but the asperities diverging from the center towards the faces, and the striæ which render the surface unequal, prove that they are formed of several crystals grouped together in the following manner.

The crystal which may be considered as the element of the group is a cuneiform octahedron. The four trapeziums, which form the four large faces, incline  $116^\circ$  on one side, and  $64^\circ$  on the other. The incidence of the triangular faces upon each other, at the place where they re-unite in a common edge perpendicular to the axis, is about  $70^\circ$ . These octahedrons are divided by sections, parallel to their larger faces. Considering these octahedrons as quadrangular prisms with dihedral summits; and supposing that their planes incline to each other in an angle of  $120^\circ$ , it is evident that three of these prisms applied together will form a regular hexahedral prism whose base, instead of being plane, will present three salient edges which will unite at the axis of the prism.

But supposing the faces of the prism to incline in angles of  $116^\circ$  and  $64^\circ$ , as is the case in nature, then three of the prisms cannot exactly fill a space; but two of them, for example, A and C, fig. 3. Pl. XV. will leave a concave space between them of 12 degrees.

To fill this space the crystallizing process employs a fourth prism D, which appears to penetrate in part the prism C, so that in the solid which results from this re-union the two faces which cut each other in the centre make an angle of  $128^\circ$ ; but as real penetration is impossible the same thing probably happens here as in those groups of crystals which crossed each other, and which at the place where they seem to enter each other, have a plane of junction situated parallel to one of the faces which would be produced by virtue of a law of diminution.

The crystals whose base is covered with cuneiform projections are nothing else but much more numerous groups of quadrangular prisms, whose respective arrangement is subjected to the same conditions.

With respect to those crystals which have four angles of  $116^\circ$ , and two of  $128^\circ$ , the order of their structure will be understood by the bare inspection of fig. 4. In this case the assemblage is composed of four prisms, A, B, C, D, which supposing no modification of form, would leave in the interior of the prism an empty space, indicated by the rhomb, *h, z, u, r*, but this is filled by an extension which each prism receives always in consequence of a law of diminution; so that the prism A increases in a quantity represented by the rectangular triangle, *h, o, z*, &c.

In the crystals whose bases are smooth a diminution takes place by a single row on the terminating edge of the component prisms.

The



The vertical section of the prisms of the arragonite present another singularity noticed by Born; it is a kind of cross, composed of four triangles, amongst which those whose bases are horizontal, are of a pale colour; and the other two, whose bases are vertical, are of a more intense violet colour.

Fig. 2. represents a prism of arragonite, where the four cuneiform octahedrons which compose it, are very distinct.

## VII.

*On the true Origin of Resin, known by the Name of Sandarac, and on that of Gum Arabic, by M. SCHOUSBOE\*.*

SANDARAC is an article of commerce, which is brought from the western provinces of the kingdom of Morocco. From six to seven hundred quintals are annually shipped in the ports of Santa Cruz, Mogador, and Saffy. This resin is called, in the language of the country *el grassa*. The tree which produces it is a *thuia*, which was also found in the kingdom of Tunis by M. Vahl, who has given a complete description, and a good figure of it in his work, entitled, *Symbol. Botan.* part the 2nd, page 96. Pl. XLVIII. by the name of *Thuia Articulata*. Shaw had formerly made it known, and called it *Cypressus, fructu quadrivalvi, foliis equiseti instar articulatis*; but neither of these learned men were acquainted with the æconomical use of this tree, probably because as it is seldom met with in the northern parts of Barbary, too little advantage can be derived from the resin which flows from it.

Till the present time this resin was attributed either to the *Juniperus Communis*, or to the *Juniperus Lycia*, or lastly to the cedar of Libanus, without considering that the *Juniperus Communis* is not found in Africa; and that the Sandarac appears to come exclusively from that part of the world. M. Schousboe, who has seen the species of *thuia* here alluded to, says, that its height never exceeds 8 metres ( $26\frac{1}{4}$  feet), and that the diameter of its trunk is not more than 20 or 22 (7 inches) centimetres. It is distinguished at first sight, from the two other species of the same kind of tree, which are cultivated in our gardens, by having a distinct trunk, and the appearance of a real tree, whilst in the others the branches issue out from the root, which gives them rather the resemblance of bushes. Its branches also are more articulate and more brittle; its blossoms, which are not very apparent, display themselves in germinal, (April), and its fruits, the form of which is nearly spherical, ripen in *Fructidor*, (September).

By presenting a branch of this vegetable to the light, it is observed to be covered with a multitude of transparent vesicles, which contain the resin. After these vesicles have

\* Extracted by Ch. Coquebert, from the Danish Journal, entitled, *Bibliot. de Physique, medicine et economie*, 3d cahier, 1799, and inserted in the *Bulletin de la Soc. Philom.* No. 31.

burst in the summer months, a resinous juice issues from the trunk and branches by exudation, as in other coniferous trees. This is sandarac. The inhabitants of the country collect it and carry it to the ports, whence it is conveyed to Europe. We make use of it in the composition of sealing wax, and for different kinds of varnish. In 1793 an hundred pounds weight of it cost in the ports of Morocco from 13 to  $13\frac{1}{2}$  piastres, which is equal to about 75 ( $7\frac{1}{2}$ d.) centimes of our money per pound. The duty upon exportation was about 9 francs per quintal.

Sandarac, if good, is of a clear yellow, pure and limpid. It is a commodity rather difficult to adulterate. Care however must be taken that the Moors do not mix too much dust with it.

It is probable the same tree which produces sandarac at Senegal, whence it is exported in rather large quantities.

Another article of commerce, of which the kingdom of Morocco also partakes with Senegal, is the gum called Arabic, which bears the name of al leilk. The tree which affords it grows only in the western provinces of that state. The exportation of this substance to the different states of Europe, amounts, throughout the ports of Morocco, to 8 or 9 thousand quintals. M. Schoufboe says, that this tree is the *mimosa nilotica*, called by the natives *al thlab*, which is no reason why in the southern parts of Africa, it may not be gathered, as authors assert that it is, from the *mimosa senegal*, and even from other species of that genus.

In Barbary, a distinction is made between the gum of Senegal and that of the country. The first has the preference, on account of its purity, its clearness, and its whiteness, which are the qualities in general sought for in this commodity.

The gum which I myself collected in the province of Mogador, observes M. Shoufboe, exudes from the trunk and the branches of the tree, like that of our fruit trees. It is in round pieces, of the bigness of a hazle nut, or at most of a walnut. These pieces, indeed, by adhering to each other, sometimes form masses of the bigness of the fist, or even of the head; but that is only owing to the adhesion which takes place between the gum, still fresh, after it had been separated, and principally from the part which adhered to the bark, where the gummy juice has not had time to harden. When earth, small stones, or other foreign bodies, are sometimes found in these masses, it is the effects of fraud. M. Shoufboe suspects that it is this circumstance which has given rise to the opinion that gum was found at the foot of trees, and that it exuded from their roots; (see Bulletin des Sciences, No. 8.) for which he thinks there is no foundation. If that was the case, it appears to him that besides the sand and the earth, with which the masses of gum are accidentally contaminated, some of it would be found in the interior part of the globules, and would be even so much blended with the mucilaginous substance, that it would not be possible to purify it completely; whereas, on the contrary, the gum which comes from Senegal is still purer than that from Barbary.

M. Schoufboe, nevertheless observes, that sandarac, and the gums which are exported through



through the port of Saffi are of a brown or red colour; but he attributes that colour to the quantity of red oxide which is mixed with the soil of the province of Abda, in which this part is situated. This oxide communicates its colour even to the whitest wool, and the inhabitants of this province are known by the red tinge of their cloaths, which no process can totally destroy.

In the months of Messidor and Thermidor, when heavy dews fall, the gum loses much of its clearness and other qualities for which it is desirable.

An hundred pounds weight of this substance cost, in the year 1793, at Mogador, about 48 francs of our money, exclusive of 5 francs 70 cents for custom-house duties.

The inhabitants of Morocco do not appear to apply this gum to any purpose whatever; they sell all that they can gather to the commercial nations of Europe.

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### VIII.

*Remarks on the Memoir in which M. Girtanner examines whether Azote be a simple or a compound Body. By Cit. BERTHOLLET\*.*

SOME experiments were long since made by Dr. Priestley, which, at first, induced him to believe, that water distilled with lime, and particularly with clay, or indeed without any addition, in an earthen retort, or a retort of glass, deprived of its polish, became changed into air. But he found that this deduction was erroneous, and that it was merely a transmission of the surrounding air which gave rise to the circumstance. Guyton added some observations to those made by Priestley (Encyclop. Vol. I. p. 674), and the fact appeared to be clearly accounted for.

Nevertheless, Wiegleb published a memoir, in which he pretended to prove, by his own, and by some other experiments, that the gas which is obtained by passing water through red hot earthen tubes might be attributed solely to the combination of the aqueous vapour with the matter of heat, and that this combination formed azote gas.

This memoir occasioned a reply which was published by Dieman, Van Tfootwyk, and Lauwerenburg. These learned chemists carefully examined every circumstance which had apparently established the pretended production of azote gas from water; and, I should have thought that there could exist no doubt respecting the consequence which they drew from their experiments, which was, "that the azote gas which, in some cases, is obtained by passing water in vapour through ignited tubes, proceeds merely from the external air, deprived of its oxygen gas by the fire in which the tubes are placed; and that, thus, the pretended conversion of water into azote gas, by its combination with the matter of heat, is overthrown."

\* *Annales de Chimie*, XXXV. 23. The memoir referred to is inserted in our Journal, and is concluded at p. 275 of the present volume.

After all these discussions, Girtanner supports the new ideas which he presents in his memoir, upon several facts, for the most part borrowed, and already accurately discussed, amongst which, the following must be considered as fundamental. "When water is boiled in a retort of glass, or other material, azote gas is obtained." The author prescribes these precautions: "In order to obtain azote gas in the greatest quantity, the water must evaporate slowly, and over a very gentle fire, which must not be raised."

"It is invariably observed in all these experiments, that when the last drop of water evaporates, the azote gas is no longer produced, though the heat be kept up."

An assertion so positive engaged me to repeat the experiment, with the precautions prescribed by Girtanner. It was made with water newly distilled, and with the well washed precipitate of alum by pot-ash. It was likewise made with a very white clay, with which I was supplied by Cit. Guyton; but though the quantity of water was considerable, and, consequently, these two experiments were of long duration, no gas became disengaged, and the result was the same as that of the Dutch chemists.

It is, nevertheless, upon this fact, which is so easy to be repeated, that Girtanner principally establishes the composition of azote, and thence that of the atmosphere, "which is not, as till now has been imagined, a mixture of oxygen gas and azote gas, but rather a mixture of oxygen gas and hydrogen—water in the form of gas, if I may be allowed to make use of such an expression. When, by those chemical experiments which very improperly have been called eudiometrical, the oxygen is separated from the hydrogen, this separation can never be entirely or completely made; a part of the oxygen remains united to the hydrogen, and forms the chemical combination which we call azote, and which we obtain in these experiments."

So that when we make a mixture of oxygen and hydrogen gas, we form atmospheric air, and the differences of specific gravity and other properties in all our experiments, as well as the products of combustion, are only slight circumstances, of which Girtanner takes no account.

The memoir of Girtanner induced Citizen Bouillon la Grange to make experiments more numerous than mine, but which lead to the same conclusions. I shall here present a short account of them, which he has kindly communicated to me.

He did not obtain any azote gas by boiling distilled water either alone, or with clay or alumine in a glass retort, to which he applied a tube of glass or of porcelain.

In like manner no disengagement of azote gas took place when the same experiment was made with siliceous earth obtained by fluoric acid.

Having placed lime, obtained from white marble, in a tube of the same kind, he passed the vapour of water through this tube, which had its extremity immersed in lime water. A small quantity of carbonic acid was disengaged, which formed carbonate of lime, but no azote gas was produced.

If, instead of lime, clay is made use of with the same apparatus, a little carbonate of lime becomes formed, but there is no disengagement of azote gas.



If water be passed through a tube of porcelain containing lead, this metal is converted partly to the state of yellow oxide, and becomes vitrified; but there is no disengagement of azote gas.

If, instead of lead, tin is put into the tube, that metal becomes oxidated, and hydrogen gas is obtained. Zinc affords the same result.

To ascertain whether this hydrogen gas did not contain azote gas, Citizen Bouillon la Grange caused it to pass over sulphur in fusion, and he obtained sulphurated hydrogen gas, but no azote. He afterwards subjected it to another trial.

He mixed the hydrogen gas which he had obtained by means of zinc, with oxygen gas; he set the mixture on fire by an electric spark; water was formed, but there was no azote gas.

The opinion of Girtanner agrees with that of Humboldt, respecting the absorption of oxygen by simple earths, and particularly by alum; there is, nevertheless, this difference, that Humboldt considers the supposed phenomenon as a simple separation of the oxygen which becomes fixed from the azote which remains in the gaseous state; but according to Girtanner, "the azote which is obtained in our experiments being always produced by operation itself, and having no existence in the form of azote before the experiment in the air which is examined, Von Humboldt, who is fond of drawing general conclusions from solitary facts appears to be deceived, when he asserts that earths may be used for the purpose of determining the quantity of azote contained in the atmospheric air: for the earths do not indicate that azote is contained in atmospheric air; they change this air into azote."

The younger Saussure has formerly contradicted the result announced by Humboldt, (*Journ. de Phys. chim. an. 7.*) He allows that the humus, which is the result of the mixture of decomposed vegetables with other vegetables which are not yet decomposed, absorbs the oxygen gas, and that this is a known fact; but he attests, "that this effect never takes place when the earths are pure and deprived of every vegetable substance." He describes several experiments which he made with alumine, lime, and silic.

In the journal of Pluviose, of the same year, there is a reply from Humboldt, written in a magisterial tone. It contains nearly the same assertions as are contained in the first Memoir with the addition, however, (as a kind of sanction), that his observations were made in the laboratories of Vauquelin and of Fourcroy. This authority would undoubtedly be of great weight, if these two learned chemists had co-operated in the observations; but very far from that being the case it is visible from the account that the only experiments, which were attempted in their presence, did not succeed.

I have been assured that the celebrated Fabroni, about the same period, repeated the experiments of Humboldt, at Florence, without any success.

Champy, the son, a very accurate observer, repeated them at Cairo upon alumine, upon lime, and upon the mud of the Nile, with a temperature which varied from 30 to 36° degrees of the centigrade thermometer, but he obtained no absorption. It must be remarked, that

that the mud of the Nile contains some remains of vegetable substances; so that, when it is distilled a certain quantity of carbonic acid and of carbonated hydrogen gas is obtained.

Chaptal repeated them at Montpellier, without obtaining absorption.

I have kept moist alumine for a considerable time in contact with atmospheric air and oxygen gas, without observing the least absorption. It having been mentioned by a chemist that absorption was caused by agitation; the effect of agitation was patiently tried, but the result was always the same. The operation has been renewed with the white clay which had been sent to me by Guyton, but it also produced no absorption. This clay, nevertheless, when urged by the fire, yielded a little carbonic acid and a small quantity of carbonated hydrogen gas.

## IX.

*On the Decomposition of the Muriatic Acid. By Mr. JOHN PITCHFORD, Junior.*

TO MR. NICHOLSON.

S. I R,

**I**F you think the following observations of sufficient importance, you will oblige me by inserting them in your useful miscellany.

I am, Sir,

Your's, &c.

Norwich, Aug. 16, 1800.

JOHN PITCHFORD, Jun.

I have long considered the analysis of the muriatic acid to be a point of the first importance in the present state of chemical knowledge. Among many experiments to ascertain its composition, which have occurred to me, one was founded on Dr. Fordyce's experiments, recorded in the Philosophical Transactions for 1792, the object of which was to discover whether, in the solution of zinc in dilute sulphuric acid, the metal derives the oxygen necessary to render it soluble from the acid, or from the water. The result of a series of very accurate experiments is well known to have been, that the water furnishes the whole of the oxygen, and that no portion of the acid is decomposed. The proof of this is that it requires as much pot-ash to precipitate the zinc from a certain quantity of acid as to saturate an equal quantity of acid in which no zinc has been dissolved.

Wishing to ascertain whether this would also be the case with muriatic acid, I dissolved one ounce of tin in four ounces of this acid of specific gravity, 1178. During the solution a considerable quantity of hydrogen gas was evolved. When the solution was completed I added pot-ash till the whole of the tin was precipitated, and the acid saturated, which took place when 1412 grains of pot-ash had been added. I next tried how much pot-ash four ounces of the same muriatic acid would require for saturation, and found that 1717 grains were required. Now allowing that some small portion of the muriatic acid, may have been



been volatilized during its solution of the tin, and carried off by the hydrogen gas during its escape, this cannot be supposed to account for more than a very small proportion of the acid which disappeared. I therefore infer that when tin is dissolved in muriatic acid, a part of the acid is decomposed, and I think it not improbable that it supplies the tin with oxygen, and that its other component part hydrogen flies off in the form of hydrogen gas. I am well aware that many more experiments ought to be tried before it can be established that oxygen and hydrogen are the constituent principles of muriatic acid; but one thing is I think fully established by this experiment, namely, that some portion of this acid is decomposed during the solution of tin: For if in Dr. Fordyce's experiment, in which two equal portions of sulphuric acid, in one of which zinc had been dissolved, and in the other not, required equal quantities of pot-ash to saturate them, it was a fair inference, that therefore sulphuric acid is not decomposed in dissolving zinc; it is equally fair to infer from my experiment in which the muriatic acid, in which tin had been dissolved, required so much less pot-ash to saturate it than an equal quantity of the same acid in which no tin had been dissolved, that part of the muriatic acid is decomposed in dissolving tin.

I am also sensible that the above experiment might be tried in a much better way: the quantity of water contained in the muriatic acid ought to be ascertained previous to the solution of the tin, and after its precipitation by the alkali, and a calculation then made, whether the portion of water missing would account for all the hydrogen and oxygen produced: if it appeared that the quantities of these were greater than the water missing would furnish, then I think it would be established that they entered into the composition of muriatic acid. Another important experiment would be to expose tin to muriatic acid gas, perfectly free from water (a method of obtaining, which appears in Mr. Henry's experiments, and will be hereafter mentioned), and examine whether it becomes oxidated, and to expose not only tin but every other metal, and every other substance, likely to separate the oxygen of this acid from the unknown radical. I am neither in possession of an apparatus sufficiently delicate, nor have I opportunity at present to try these and similar experiments, but having seen in the Philosophical Transactions for 1800, page 188, that Mr. William Henry has been engaged in experiments on the decomposition of this acid, I have thought it right to suggest the above hints.

The chemical world has great obligations to Mr. Henry for the labour and patience he must have expended on his very interesting experiments. I should wish to be permitted to make a few observations which resulted from an attentive perusal of his paper.

The first series of his experiments consisted in passing the electric shock repeatedly through muriatic acid gas confined over mercury. The effect was the diminution of this gas, the production of hydrogen gas, and the oxidation of the surface of the mercury: all this he attributes not to any decomposition of the acid, but to that of the water, which in the dryest form, in which the gas is usually produced, still adheres to it. As these effects took place only for a limited time, after which continued electrification produced them no longer, and as a portion of muriatic acid gas always remained entire, this inference

is.

is probably true, and the experiments prove that the electric matter at least is incapable of decomposing muriatic acid. But though no progress may have been made towards the decomposition of the acid by these first experiments, they are yet far from being useless, since we are now supplied with the means of obtaining the muriatic acid perfectly free from water, namely, that portion which, after repeated electrification, remains unaltered. I have no further remark to make on the first series of experiments, except that there appears to be some obscurity in the 5th, in which 143 measures of common air, and 116 of muriatic acid gas, were electrified, and by 30 shocks reduced to 111. "The remainder consisted of muriatic acid and azote gases, with a small proportion of oxygenous gas." What, then, became of the hydrogen gas?

The second series of experiments was to ascertain the effect produced by electrifying the muriatic acid gas with inflammable substances. Mr. Henry previously observes, that he has attempted the decomposition of this acid by passing it over red hot charcoal. "An immense production of hydrogenous gas took place, but it was not easy to determine whether it had its origin from real acid or from water." I cannot help being surprised that Mr. Henry should not have decided this point by passing muriatic acid gas entirely freed from water by electrification, over red hot charcoal, as this was so obvious a method of ascertaining whether charcoal be capable of decomposing muriatic acid.

In the 12th experiment, Mr. Henry passed 200 shocks through a mixture of 83 measures of carbonated hydrogen gas, and 89 of muriatic acid gas. The permanent residue, after the admission of water, was 101 measures; the addition, therefore, amounted to 18; of these 6 may be accounted for by the decomposition of the water of the muriatic acid gas, and 10 by that of the carbonated hydrogen gas. There remain, therefore, only 2 measures that can be supposed to be produced from the muriatic acid gas; a quantity, Mr. Henry observes, too small to afford grounds for supposing them to arise from decomposed acid. Now, in the decomposition of the water of the carbonated hydrogen gas by the carbon, the oxygen of the water unites with the carbon, and forms carbonic acid, which being soluble in water, would on the admission of water, after the electrification, wholly, or in part, disappear; so that the increase is greater than 2, by all the carbonic acid dissolved in the water. It is not, therefore, improbable that the muriatic acid gas was in part decomposed in this experiment, and that the reason why more of it was not decomposed is owing to there not having been more charcoal present, part of what there was being employed in decomposing the water. Indeed, when it is considered that the carbon in carbonated hydrogen gas is minute in quantity, and that it has to decompose the water contained in the two gases before it can act on the muriatic acid gas, it appears that this is not the best method of ascertaining whether carbon can decompose muriatic acid. There appears to me no better method than to pass muriatic acid gas, perfectly freed from water, over red hot charcoal, and to examine whether carbonic acid and hydrogen are produced.



## X.

*Observations on the Action which the Sulphate of Iron exercises upon Nitrous Gas, and on the Formation of Muriatic Acid. By Citizen BERTHOLLET\*.*

**M.** HUMBOLDT (in the Bulletin des Sciences, No. 71 & 21) had asserted that the faculty which sulphate of iron possesses of absorbing nitrous gas without the azote which might be mixed with it, was a means of ascertaining with precision the purity of the nitrous gas employed in eudiometrical experiments.

Citizen Berthollet, on the contrary, is of opinion, that in this case the nitrous gas is not only absorbed, but that it is decomposed; that the oxygen abandons a part of the azote, and forms, with the other part, nitrous acid; that this decomposition equally takes place by means of water, mercury, solution of pot-ash, or of hydrogenated sulphuret of alkali; that it is more or less complete in proportion as the liquid, in contact with this gas, contains bodies which have a greater degree of affinity to nitrous acid, in the formation of which they assist, and nitrites are formed; that in the decomposition by water, there is less nitrate of ammoniac and more nitrous acid formed than M. Humboldt imagined; which tends to prove that water did not furnish the whole of the oxygen of the acid, since the quantity of acid thus formed, is far more considerable than may be presumed from the existing quantity of ammoniac. It is to be remarked that the decomposition becomes more difficult as it is more advanced, and the gas contains less oxygen. Citizen Berthollet attributes the differences which are observable in nitrous gas, to the different proportions in which the oxygen and azote are combined, and he does not think they are owing to azote in a state of simple mixture.

It is a known fact, that oxygenated muriatic acid, which has no action upon azote, absorbs nitrous gas very well. M. Humboldt observed in this absorption a residue, which he attributed to azote mixed with the nitrous gas. Citizen Berthollet having repeated the experiment with nitrous gas, carefully prepared, found a residue so small as not to deserve notice.

In a word, Citizen Berthollet restores to hydrogenated sulphuret of pot-ash and to phosphorus the property of taking up all the oxygen of the atmospheric air; a property which had been contested by M. Humboldt, who says, that nitrous gas always indicated the existence of a residue of oxygen in air which had been submitted to their action. Citizen Berthollet absolutely maintains the contrary. Nitrous gas experienced only a slight diminution with the residue of atmospheric air decomposed by phosphorus acid; and he attributes this to the absorption which takes place in the nitrous gas of that portion of phosphorus which had been taken up by the azote.

\* Abridged from the Memoirs communicated to the French National Institute (in the Bulletin de la Soc. Philom. No. 40.)

*On the Nature of Muriatic Acid.*

Two circumstances induced Citizen Berthollet to entertain suspicions, and to investigate the nature of this acid.

The one is that which is announced in the Bulletin, No. 17, page 165, where M. Humboldt says, that muriate of iron is formed by the absorption of nitrous gas, by means of sulphate of iron.

The other, which had been observed by Cavendish, is the precipitation of nitrate of silver into muriate, by the nitrite of pot-ash obtained from nitrate of pot-ash in part-decomposed by fire. By adding to these observations the circumstance of the almost invariable presence of muriatic acid in every case where nitric acid is formed, and by means of several other experiments carefully made, Citizen Berthollet was led to the discovery of the nature of muriatic acid.

He ascertained that nitrous gas does not precipitate the solution of silver. By repeating the experiment of Cavendish, with nitrate of pot-ash, he found it accurate, and was moreover assured that the precipitation could not proceed from the nitrous gas of the nitrite; for,

1st, Nitrate of lime does not produce the same result.

2d, If a solution of iron be made in nitric acid, and it be so managed as to load it with this metal, little ammoniac is formed; the solution becomes turbid, and it does not precipitate the solution of silver. If a fresh quantity of iron be added to this solution, there is an effervescence, with a precipitation of almost all the oxide of iron, the liquid contains more ammoniac, together with muriatic acid, which is easily demonstrable by the solution of silver. If this liquid be distilled, that which comes over contains nothing but ammoniac; the muriatic acid remaining in the retort with part of the ammoniac.

3d. Nitric solutions of tin, of zinc, of copper, made by heat, have sometimes yielded muriatic acid. But it is to be remarked that this acid was more generally found in proportion as the production of ammoniac was considerable.

In these experiments there are anomalies, the cause of which citizen Berthollet has not yet been able to assign. But they already prove that muriatic acid is formed under these circumstances, and that its formation cannot be attributed to the presence of pot-ash. The principles of this acid must therefore be sought for in water and nitric acid.

Its incombustibility, its resistance to decomposition, prove, that if it contains hydrogen and oxygen, these two bodies are not predominant, for it is a principle of the theory of affinities, that the difficulty of destroying a compound is in proportion to the smallness of the quantity of one of its component parts. Almost all the proportions of the combinations of azote and oxygen are known. Citizen Berthollet, therefore, supposes himself to be justified in thinking that muriatic acid is a triple composition of oxygen, of hydrogen in a small quantity, and azote in a greater proportion. By adopting this opinion, the presence of muriatic acid, in a great number of chemical phenomena, is easily explained.



Thus it appears (experiment 2.) that it is at the moment when the iron has decomposed almost all the nitric acid, and it becomes necessary also to decompose the water, in order to oxidate more metal; that the muriatic acid, and the greatest part of the ammoniac, is formed by the decomposition of the water.

It is to the presence of oxygen, azote, and hydrogen, that citizen Berthollet attributes the formation of muriatic acid in artificial nitre beds, when the materials do not previously contain any muriate.

Although muriatic acid, from the proportions of its constituent principles, ought strongly to resist decomposition, citizen Berthollet thinks that he has observed that this decomposition takes place under some circumstances.

He thinks that the residue which is left by oxygen gas disengaged by heat, from oxygenated muriate of pot-ash, is owing to the decomposition of a small portion of this acid. He had, at first, attributed this residue to a foreign cause; but having remarked that it was more abundant at the end than at the beginning of the operation, he conceived it could not result from such a cause.

Citizen Berthollet closes his Memoir, by shewing, from more accurate experiments, that the black colour which muriate of silver assumes from light, heat, and even from a simple current of air, ought not to be attributed, as he himself had supposed, to a gaseous disengagement of oxygen, but to the separation of a part of the muriatic acid, without decomposition.

## SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

*Philosophical Transactions of the Royal Society of London, for the Year 1800. Part the Second. Quarto, 46 Pages, with Eleven Plates. London, sold by Elmsley.*

**T**HIS part contains.—12. On double images, caused by atmospherical refraction. By William Hyde, Wollaston, M. D. F. R. S. (see our present, vol. p. 298.) 13. Investigation of the powers of the prismatic colours to heat and illuminate objects; with remarks, that prove the different refrangibility of radiant heat. To which is added, an enquiry into the method of viewing the sun advantageously, with telescopes of large apertures and high magnifying powers. By William Herschel L. L. D. F. R. S. 14. Experiments on the refrangibility of the invisible rays of the sun. By William Herschel, L. L. D. F. R. S. (abridgment of these two papers is given at p. 320) of our present volume. 15. Experiments on the solar, and on the terrestrial rays that occasion heat; with a comparative view of the laws to which light and heat, or rather the rays which occasion them, are subject, in order to determine whether they are the same, or different. By William

Herschel, L. L. D. F. R. S. (see p. 360 of this number). 16. Chemical experiments on zoophytes; with some observations on the component parts of membrane, by Charles Hatchett, Esq. F. R. S. 17. On the electricity excited by the mere contract of conducting substances of different kinds. In a letter from Mr. Alexandar Volta, F. R. S. professor of natural philosophy in the university of Pavia, to the Right Hon. Sir Joseph Banks, Bart. K. B. F. R. S. (abridged in this Journal, IV. p. 179.)

*Fossil Wood, found at a very great Elevation.*

Citizen Villars has communicated a Memoir to the national institute of France, in which he relates his having seen, in the department of *Isere*, fossil wood, bedded in turf, at the height of 2320 metres above the (actual level of the sea, and 850 metres above the most elevated situation, where wood grows, at present. The mountain where he has made this interesting observation, is that of Lans, in the canton of Aisans. The trees which are found are The roots, and parts of the trunk are observable. The latter of these trees no longer grows in the vicinity.

The author attributes the refrigeration which these mountains have experienced to two leading causes; the first is the excavation of the valleys, which has changed the elevation of the summits relative to their base, and the surrounding country; and the second is the destruction of the ancient forests, which extended contiguously upwards to great heights, but cannot again grow at similar elevations, when once destroyed, and the trees thus deprived of their mutual shelter and defence. Soc. Philom. No. 33.

*Extract of a Letter from Mr. Humphry Davy, dated October 23. supplementary to his Paper on Galvanism, in the present Number.*

When I exhibited to Dr. Beddoes the phenomenon of the renovation of the powers of the galvanic pile, after it had ceased to act in hydrogen, nitrogen, &c. by momentary immersion in water, holding in solution atmospheric air, he requested me to try whether oxygenated muriatic acid gas, solution of nitre, &c. would not increase the effects. The fear of destroying the silver prevented me at the time from trying the experiment on oxygenated muriatic acid gas. I have since, however, in endeavouring to ascertain whether the electrical phenomena of galvanism were producible when the plates of the pile were oxidating, (separated from each other by a non-conductor, and in contact with an aeriform fluid only) had an opportunity of observing the very great power of this substance in producing the galvanic decomposition of water.

Ten pairs of plates of silver and zinc were connected by cement, so as to prevent the alternate contact of the metal. The spaces filled by cloths in the common pile, were

offered.



suffered to remain open, so as to admit of a free circulation of air. A tube, with water, and silver wires, was adapted to the extreme plates.

This pile was introduced, without being moistened, into a vessel, provided with a stopper, filled with oxygenated marine acid gas; but no perceptible galvanic action took place. After two hours, no gas had formed in the tube, nor had any oxidation of the zinc wire been produced\*. The same pile was now moistened by immersion in water. Before and after it had been wiped, it shewed no signs of action in the atmosphere. It was introduced into a vessel of oxygenated marine acid gas, opened as before in the atmosphere. In a moment, the zinc wire, in the tube, began to oxidate with the greatest rapidity, whilst gas was given plentifully from the silver wire. The process continued to go on till the green colour had disappeared in the cylinder.

This experiment not only arranges with the facts of Fabroni and Colonel Haldane, and those I have before stated; but likewise seems to prove that the chief use of the large surface of water required in the pile of Volta is to oxidate a larger quantity of zinc: for in this instance very minute quantities of water connected the plates, and consequently very minute quantities were sufficient to enable the electrical currents to form the circuit.

I am at present engaged in endeavouring to ascertain by experiments whether any differences exist in the gases evolved from water by the galvanic current, when different oxidating substances form the medium of communication between the plates. When these experiments are completed, or at some future time I shall probably offer some observations on the peculiar affinities which enable iron, zinc, &c. to decompose water only when it holds in solution atmospheric air, acids, or other bodies containing oxygen. On the principles before laid down, nothing is more easy than to explain the use of muriate of soda, muriate of ammoniac, &c. in increasing the powers of the common pile.

As the quantity of power in a pile is probably in proportion to the quantity of the oxidation of the zinc, and the number of the series of plates, the pile of Mr. Cruickshank, or that I have described, will probably be most useful for processes, in which much galvanic power is required. In those processes, muriatic acid, or very diluted nitrous acid, may be used as the oxidating substance: for they will enable the plates to act till all the oxidable metal is destroyed, without the common trouble of cleaning and rebuilding the pile.

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#### *Analysis of various Fossils.*

Trommsdorff, who has lately been engaged in mineral analyses, has found, that the lapis obsidianus of mount Heckla in Iceland is composed of 63 parts of siliceous earth, 20.5 parts of alumine, and 13.5 parts of oxide of iron.

That the heliotrope of Bohemia contains 68 parts of siliceous earth, 15 of alumine, 10 of oxide of iron.

\* The metals of the pile had been acted upon in this experiment, and were warm at the conclusion of it.

That

That the black felspar which is found at Unkel in the Bafaltes, is composed of 66.5 parts of alumine, 15.0 of filiceous earth, 6.5 of oxide of iron, and 4.0 of oxide of manganese.

That the blue calcedony of Siberia contains only filiceous earth.

That rock crystal is the same as pure filiceous earth.

That the Holzstein of Bareuth contains 80.0 parts of filiceous earth, 11.0 of alumine, 3.0 of oxide of iron, 1.0 of oxide of chrome; which proves that chrome is also found in Germany. He suspects that it is likewise to be found in several other of the fossils of this country.

#### *Lime in the Nut-gall.*

Proust did not obtain lime from the nut gall; Trommsdorff ascertained the presence of this earth with precision.

The method which Proust proposes to procure pure gallic acid is not advantageous; at best it is good for separating this acid from the tanning principle, but it does not disengage it from the extractive matter, and when it is proposed to separate the muriatic acid by evaporation, the elevated temperature which is necessary to effect it, resolves the gallic acid into its principles, or at least carbonifies it.

#### *Extraction of the Alkali of Sea Salt by Lime.*

The decomposition of sulphate of soda by lime in order to obtain that alkali cannot be advantageous, because this earth attacks the salt only in proportion as it is itself dissolved in water.

#### *Method of obtaining Phosphorus by Phosphate of Lead.*

Trommsdorff has made new attempts to render Giobert's process useful, by precipitating urine by nitrate and acetate of lead; but the phosphorus which he obtained was in such small quantities, that it did not pay the expence of the fire. The chemist of Turin would be justified in affirming that his method is the best, if his precipitate had been the pure phosphate of lead. But the reduced lead which remains in the retort is the phosphuret of that metal.

#### *On the Mineral Kermes, and the golden Sulphur of Antimony.*

Several new experiments have also been made by the same chemist, on the red and orange coloured hydro-sulphurets of antimony, and he is perfectly satisfied that the former consists of oxide of antimony and sulphurated hydrogen; and that the latter is composed of the same principles, and a portion of sulphur not hydrogenated. By directing a current



rent of sulphurated hydrogen gas into nitro-muriate of antimony, kermes immediately falls down, and afterwards when there is a sufficient quantity of acid at liberty to dishydrogenate the sulphur, golden sulphur precipitates. The same gas converts all the antimony contained in the acetite, and in the tartrite of this metal into kermes, which process affords the means of procuring this preparation of a nature perfectly uniform. \*

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*Elastic Resin in Opium.*

M. Bucholz, a friend of Trommsdorff, has lately addressed a very curious memoir to him upon opium, which he means to insert in his next Journal. He found, in particular, that opium contains a great quantity of elastic gum.

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*Discovery of a New Earth in the Beryl of Georgen-Stadt.*

During the last ten years a mineral has been found in the mines near Georgen-Stadt, to which, on account of its resemblance to the beryl, the name of that fossil had been given. Trommsdorff undertook to analyze this substance in the hopes of finding the glucine, but instead of that earth he found a new one, or one which was different from those hitherto known.

1st. In its pure state it resembles alumine. 2. It is not more soluble either in the dry, or humid way in the caustic alkalis, than in their carbonates. 3. Ammoniac, whether caustic or carbonated, exercises no action upon it. 4. It retains the carbonic acid but weakly. 5. It acquires hardness, but not sapidity by fire. 6. It is not soluble in water. 7. It readily unites to acids with which it forms salts, which have little or no taste. 8. The earth, hardened by fire, dissolves in the acids with the same ease as that which has not been so treated. 9. It forms with sulphuric acid a salt little soluble, perfectly insipid, which when it is acidulated dissolves without difficulty, and becomes crystallized in stars. 10. Superaturated with phosphoric acid it affords a very soluble salt. 11. Its acetite is of very sparing solubility. Its other characteristics, as well as his process of analysis, will be detailed in the 1st part of the 8th Volume of his Journal. He has given the name of *Agustine* to this new earth, from its property of forming with acids salts without taste.\*

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*On the Method of hearing by the Teeth.*

Citizen Vidron, music master at Paris, has announced the discovery of a method of rendering music audible by persons deaf and dumb from their birth.

Citizens Hauy, Lacépède, and Cuvier, who were appointed by the National Institute to examine this discovery, made their report (the 21 Messidor) in the year 8 (July 21, 1800.)

\* See Annales de Chimie XXXII. 257, for an account of all the circumstances which determine the formation of the different sulphurets of antimony. French editor.

† The last seven articles are translated from a letter of Trommsdorff in the Annales de Chimie XXXIV. 130.

Citizen Vidron uses a rod of steel, one end of which he places upon the sound-board of the musical instrument, and the other between the teeth of the deaf person. He adds to it a branch terminated by a brass button which rests upon the cavity of the stomach, and sometimes a third which he places upon the head.

The commissioners have found that many authors have announced the fact of the deaf being made to hear by putting their teeth in communication with the instrument by means of a stick, a goblet, or some other body. They have quoted, amongst others, Fabricius D'Aquapendente, Schellhammer, Boerhaave, Winkla, and Jorissen.

They have also found that in like circumstances steel is better for the purpose than wood, which had been almost generally used before the time of citizen Vidron; but that his two branches are of no use with respect to what may properly be called hearing.

They particularly endeavoured to determine to what extent this method might be useful; as well with respect to the different kinds of deafness, as to the different kinds of sounds which are proposed to be rendered audible.

They produced an artificial deafness in themselves by stopping their ears, and retiring to a distance. In both cases they heard distinctly by means of the steel rod, and the sounds appeared to them to proceed from within this rod, and not from their true place.

But the persons, really deaf, whom they have examined presented very different results. Some of them manifestly heard; but the greater number declared they only experienced a tremulous action more or less general.

The commissioners conclude, that this method may be serviceable in deafness which only proceeds from obstructions in the external passage, but that it is useless in that which is caused by a paralysis of the nerve, or an essential derangement in the interior part; which is the most common kind of deafness, especially in those who are deaf from their birth. They think it proper, nevertheless, to make the experiment on all young persons who are deaf, since if only one out of an hundred should derive benefit from it, to that one at least it would be a source of enjoyment.

As to articulated sounds or speech the commissioners found that it was almost impossible to hope for an exact transmission of them by this method, at least in its present state.

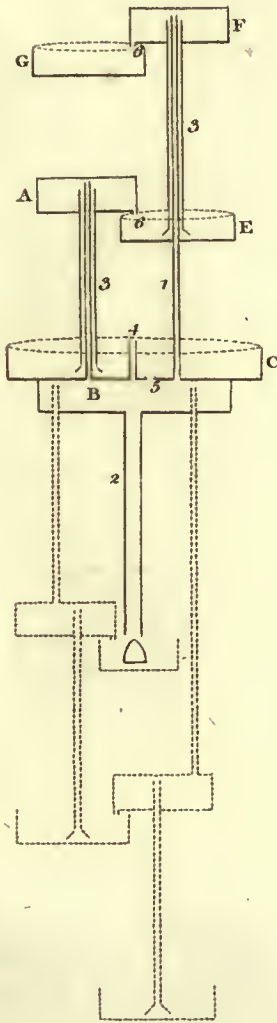
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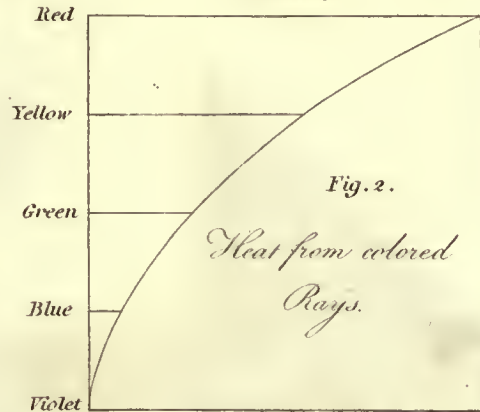
\* \* \* Mr. Leslie has favoured me with a correction of his paper, where page 346, line 19, for *public opinion* the words *popular opinion* are to be substituted. And he fears that other verbal inaccuracies may have occurred, as the copy was written in haste, and the unexpected absence of the author, during the impression, prevented any subsequent revision.



*Fig. 1.*



Intensity.

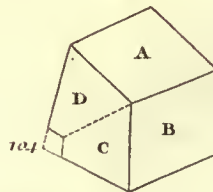


*Fig. 3.*

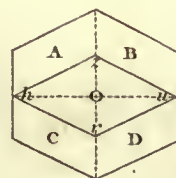


*Aragonite*

*Fig. 4.*



*Fig. 5.*

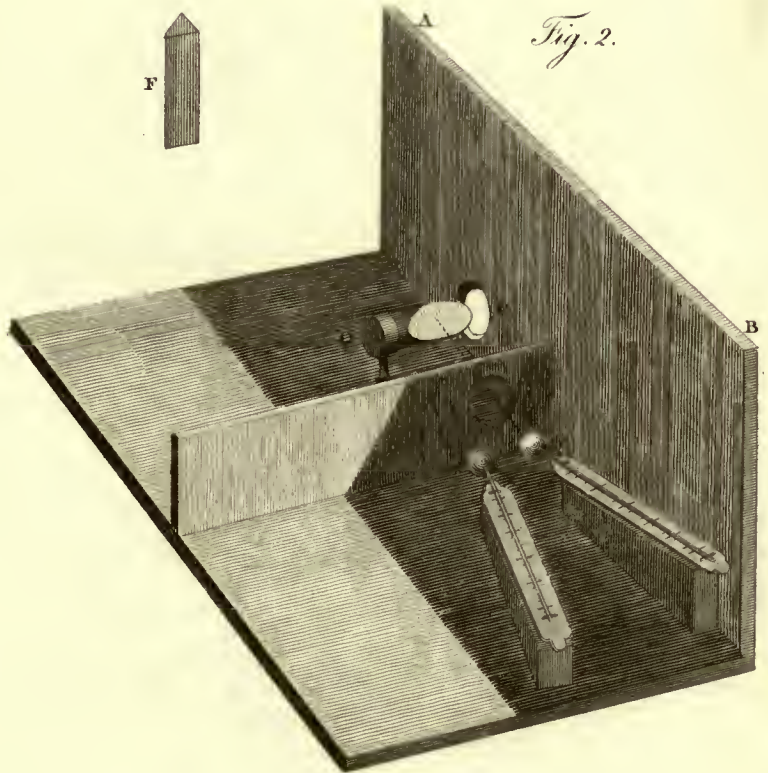




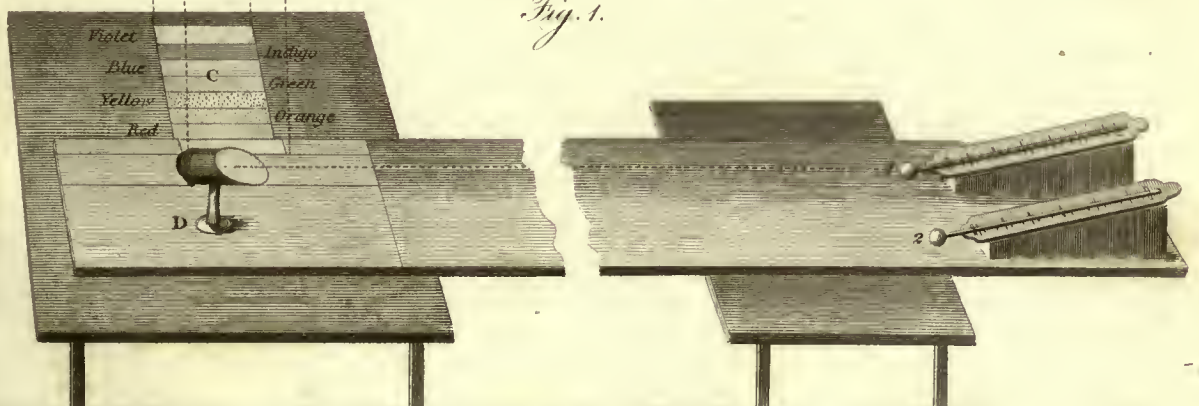


*Radiation of heat.*

*Fig. 2.*



*Fig. 1.*







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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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DECEMBER 1800.

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ARTICLE I.

*On the Number of the Primitive Colorific Rays in Solar Light. By the Rev. MATTHEW YOUNG, D. D. S. F. T. C. D. & M. R. I. A.*

THE opinion that there are but three primitive colours has been maintained by M. du Fay, and after him by Father Castelli. See Montucla, Vol. I. p. 630.; but they and all others who hold the same doctrine, defend it merely on the principles of a painter, who shews how with these three colours on his pallet, he can compound all others; for with red and yellow he can form an orange colour; with blue and yellow he forms green; and with blue and red he forms indigo and violet; and thus having compounded the seven prismatic colours, it is manifest that all other colours, with their different gradations, can be formed from them likewise. But this *pharmaceutical* argument is by no means sufficient to satisfy us as to the real composition of solar light.

“Light, in refracting, is decomposed into seven rays, red, orange, yellow, green, blue, indigo and violet. It has been supposed,” says Fourcroy, “that three of these colours, the red, yellow, and blue, were simple; and that the other four were formed each of its two neighbours; that is, the orange from the red and yellow, the green from the yellow and blue, the indigo from the blue and violet, and the violet from the red and indigo. But this supposition has never been proved.” See his *Philosophy of Chem.*

chap. 1. § 3. Besides that this is a mere hypothesis, unsupported by any fact, as Fourcroy observes, we remark, that it is in itself inadequate; 1st, because in the solar spectrum, the red and indigo are not *neighbouring* colours, but are almost at the greatest possible distance from each other. 2dly, According to this hypothesis, indigo is composed of blue and violet; but violet is composed of red and indigo; indigo therefore is composed of red, blue and indigo, that is, indigo itself is one of its own essential ingredients, which is absurd.

The experiments of the prism seem to establish, in a very clear manner, the existence of seven original and uncompounded colours; and though green, for instance, may be compounded of blue and yellow, yet it does not directly follow from thence, that it always is so actually compounded. Accordingly Newton tells us, that green may be exhibited in two different ways, either by primitive, green-making rays, which are simple and not resolvable by any reflection or refraction into different rays; or by a composition of blue and yellow rays, which are differently refrangible, and which therefore after their union, may again be separated by refraction, and exhibit their proper colours of blue and yellow.

On this doctrine of the two-fold generation of green, we may in the first place remark, that the ancient, received axiom "*Deus nil agit frustra*" ought not to be too hastily abandoned, as it must appear to be, if this doctrine be maintained: for if green may be produced by blue and yellow, then blue and yellow being already existent, green is a consequence; and therefore peculiar rays formed for the production of green are superfluous. Though I acknowledge, that this maxim is not so cogent or self-evident, as to preclude all objection, yet since the general observation of nature seems to shew, that this waste of power or multiplicity of means is not adopted by the Supreme Artist, it certainly seems justly entitled to our attention, at least so far as this, that we should be careful in shewing, that we are led to these different causes of the same effect, by a legitimate and cautious analysis.

In defence of the doctrine of three primitive colours only, F. Castelli contents himself with saying, that the colours of the prism are immaterial, accidental, artificial, and therefore unworthy the regard of a philosopher; whereas the colours of painters are substantial, natural, palpable. From them, of consequence, the theory of chromatics should be deduced; but they tell us, that there are but three parent colours, which give birth to all others.

In reply to this we need only observe, that Sir I. Newton has proved, that the colours of natural bodies depend on the colorific qualities of the rays of light; and therefore that our theory of colours must be derived from an enquiry into the constitution of solar light, for according to that constitution the colours of bodies will vary: and he farther shews, that if solar light consisted of but one sort of rays, all bodies in the world would be of the same colour. However true therefore F. Castelli's theory may be, the manner in which he deduces it from phenomena is unquestionably false.

I shall therefore proceed to enquire scrupulously into the composition of the solar spectrum, from which, without doubt, the true doctrine of the origin of colours is to be derived.

If



If the solar light consisted of seven primitive, homogeneous coloured rays, and that these homogeneous rays were equally refrangible, the spectrum would consist of seven circles of different colours, since the homogeneous rays of each colour would paint a circular image of the sun. But it is manifest, that seven circles could not compose an oblong spectrum, with rectilinear sides. Therefore the rays of the same denomination of colour must be differently refrangible. Which is also made still farther evident by observation of the spectrum, since in it we perceive, that the prismatic colours are diffused over spaces, which are, on the sides, terminated by right lines, and therefore the centers of the circles of the same denomination of colour are diffused over lines equal to these segments of the rectilinear sides of the spectrum. Newton has shewn, prop. 4. B. 1. Optics, how to separate from one another the heterogeneous rays of compound light, by diminishing the breadth of the spectrum, its length remaining unchanged; and when the length of the spectrum is to its breadth, as 72 to 1, the light of the image is seventy-one times less compound than the sun's direct light. In the middle of a black paper he made a round hole, about a fifth or a sixth part of an inch in diameter, upon which he caused this spectrum so to fall, that some part of the light might pass through the whole of the paper; this transmitted part of the light he refracted with a prism placed behind the paper, and letting the light fall perpendicularly upon a white paper, he found that the spectrum formed by it was perfectly circular. Hence, therefore, it follows, that the equally refrangible rays occupy a space on the rectilinear sides of the spectrum equal at least to the fifth or sixth part of an inch; that is, the rays of the same colour are differently refrangible.

The different quantity of the homogeneous rays of different colours will not account for the different spaces they occupy in the spectrum; for this difference in quantity would affect only the intensity of the colour, not the magnitude of the space which it would occupy. All the red light therefore is not homogeneous; but consists of rays of innumerable, different degrees of refrangibility; and so of the other colours.

Now since the rays which are of the same denomination of colour are differently refrangible, they will either form oblong spectrums detached from each other; or they will in part lap over, and fall on each other. The former position is manifestly false: therefore the original prismatic colours will partly lap over and fall on each other, and therefore necessarily generate the intermediate colours. And so Sir I. Newton observes, where he says, that the original, prismatic colours will not be disturbed by the intermixture of the continuous rays, which are intermixed together. This overlapping however, which Newton speaks of, arises only from the sun's having a sensible diameter, and does not necessarily imply an equal refrangibility in any differently coloured rays. If there be but three original prismatic colours, red, yellow, and blue; and that the red and yellow lap over, so as that there shall be a certain space in the sides of the spectrum equally occupied by yellow and red circles, then will these circles by their intermixture compound an orange colour; and this colour as to refrangibility will be homogeneous, because the coincident rays of different colours are equally refrangible. In like manner green may be compounded by the

mixture of blue and yellow circles, equally refrangible. Now this is simple, and conformable to the other phenomena of the spectrum; for if rays of the same denomination of colour be differently refrangible, it is not unreasonable to suppose, that rays of a different denomination of colour may be equally refrangible; and therefore since the red rays are unequally refrangible, and likewise the yellow, there is nothing incongruous in supposing that some of the less refrangible of the yellow may be equally refrangible with some of the more refrangible of the red; and if so, they will consequently be intermixed with them: and the same may be said of the green. This hypothesis likewise receives considerable strength from this consideration, that the orange, green, indigo, and violet occupy those places which they ought to do, in case there were but three primitive colours, red, yellow, and blue: thus the orange lies between the red and yellow, because it is formed by some of the extreme rays of red and yellow, which are equally refrangible; in like manner the green lies between the blue and yellow, because it is formed by the mixture of blue and yellow. The indigo and violet must also occupy the extreme part of the spectrum, where the most refrangible red and blue rays are united, and gradually becoming more and more dilute, fade away, and at length entirely vanish. But if the orange, green, indigo and violet be primitive colours, there is no apparent reason why they should have had such degrees of refrangibility assigned them, as that they should occupy the places they do, rather than any other.

Moreover, if these three colours red, yellow, and blue be the primitive colours, they cannot themselves be generated; and accordingly we find, that yellow cannot be generated by the mixture of the adjacent prismatic colours, orange and green; and the reason of this is evident, because orange is compounded of red and yellow; and green is compounded of yellow and blue; but red and blue compose purple; which added to the yellow will generate a new compound colour, viz. a sickly green, differing manifestly from yellow, the colour which ought to result according to the analogy of the other primitive colours, in which the extremes, by their mixture, generate that which is intermediate. In the same manner, blue cannot be generated by the mixture of green and indigo, because green is composed of yellow and blue, and indigo of blue and violet; therefore the resulting colour is composed of blue, yellow, and violet; but yellow and violet do not compose blue, therefore neither will blue, yellow, and violet compose a blue colour. Now if orange and green be primitive colours, in the same manner as red, yellow, and blue, we can assign no reason why blue should not be generated by the mixture of the adjacent colours, as well as green and orange. But it is a received principle, that an hypothesis should solve all the phenomena; of the two hypotheses therefore, namely, that there are seven primitive colours, differently refrangible; or that there are but three, some of which, of each species, are equally refrangible; the latter alone solves all the phenomena of the solar spectrum, and therefore is to be preferred.

If it be said, that those rays which are equally refrangible must excite the same sensation on the retina, because they must have the same momentum; it is replied, 1st, That it has  
not



not yet been proved, that the sensation of different colours depends on the different momentum of the rays. 2dly, The rays may have different momentums, and yet be equally refrangible; for since refraction is supposed to depend on the attractive force of the denser medium, we must suppose it analogous to the attractive force of gravity, which is proportional to the quantity of matter; and therefore the greater or less quantity of matter in a particle of light would produce no alteration in its refraction. Neither can the different refrangibility depend on the different velocity of the rays; because the difference of refrangibility of the red and violet rays is much greater in flint glass than in crown glass; and this would require a proportionably greater difference in the original velocities, which cannot be. And this same argument holds equally against the former hypothesis, that the difference of refrangibility depends on the different magnitude or density of the particles of light. 3dly, Refraction seems to arise from a species of elective attraction, since different mediums which act on the mean rays equally, act on the extreme rays unequally: hence rays of the same quantity of matter and velocity, and therefore of the same momentum, may be diversely refracted; and rays of different momentums equally refracted.

Nor is it to be wondered at that the rays of light should be differently refrangible, independent of any regard to their momentum, when we consider, that the different coloured rays appear to be combined with combustible bodies, with different degrees of attractive force. For in combustion we find, that different bodies are disposed to part with different rays with greater facility; but when the combustion is sufficiently rapid, they part with all the different coloured rays together, and the flame is therefore white; and this is what is called a white heat. Dr. Fordyce in the *Phil. Trans.* for 1776, tells us, that when the heated substances are colourless, they first emit a red light; then a red mixed with yellow, and lastly, with a great degree of heat, a pure white. All this is wonderfully conformable to the refraction of light by transparent substances, which refract, and therefore attract the red light less, and consequently in combustion part with it more easily. On the other hand I know it is generally believed, that the light in combustion proceeds from the air, but this circumstance of the different colour of the light in different cases, seems to overturn this opinion; for if vital air were oxygen dissolved in caloric and light, then the oxygen being absorbed by the burning body, the light extricated would in all cases be of the same nature; the greater or less rapidity of the combustion would only produce an extrication of a greater or less quantity of light, but could not produce any variation in its nature, it being necessarily the same in all cases, to wit, that in which vital air is dissolved. But the truth or falshood of this reasoning will not affect the validity of the position, that the refrangibility of the rays of light cannot depend on the different magnitude, density, or velocity of the particles.

But though speculation seems thus to render it probable, that there are but three parent colours; our theory must ever remain unsatisfactory, unless it receives the sanction of direct experiment. In this however there is no small difficulty; for since the rays of light which compose any given individual point of the colours of orange, green, violet, and indigo are

equally

equally refrangible, they will be also equally reflexible; and therefore cannot be separated either by refraction or reflection, so as to exhibit the different coloured rays of which they are composed. It seems therefore, that the only way remaining, by which we can experimentally ascertain the composition of these colours, if they be indeed compound, is transmission. For since transparent coloured bodies are such merely by their letting pass through them either solely, or more copiously, rays of a certain colour, and intercepting all others, such transparent bodies, applied to compound colours, will ascertain that composition, by extinguishing, in a great measure, all rays except such as are so adapted to its conformation, as to pass through it, and give it its peculiar denomination of colour.

In order to try the truth of the hypothesis of seven colours by this test, I looked through a blue glass at the red end of the spectrum: now we are to consider, that if that part of the spectrum was composed of red rays, and none other, the only effect of the blue glass would either be a total or partial suffocation of the red rays; and therefore that part of the spectrum, when looked at through the glass, would either totally disappear, or become a faint and diluted red. But, on experiment it appeared of a purple colour. The purple in this case could not be a primitive and original colour, as is manifest, because it did not proceed from the purple part of the spectrum; we must therefore conclude, that it was a compound colour. But purple, when compound, is made up of blue and red, therefore it follows, that some blue rays did actually exist in the red part of the spectrum: which combined with the few, straggling red rays which penetrated the blue glass, composed that purple colour, which the red extremity of the spectrum assumed, when viewed by the light transmitted through the blue medium.

To try, on the other hand, whether any red rays lay hid amongst the blue, I proceeded in the same manner, and looking at the bluest part of the spectrum through a red glass, it appeared of a purple colour: some red rays therefore are equally refrangible with the blue; and if the red extends as far as the blue, there is no reason why we may not suppose that it extends somewhat farther, so as to compound, with a diluted blue, the extreme colours of the spectrum, indigo and violet.

But it may be said, that if blue rays existed amongst the red, that part of the spectrum could not appear so extremely brilliant as it really does; but would put on a purplish appearance in the spectrum itself, even to the naked eye. In answer to this objection we may observe, that the most intense and vivid, natural red bodies do, in fact, reflect a very great proportion of blue rays, because they appear of a strong blue colour when placed in the blue part of the spectrum; and therefore they reflect just as many when the direct, white solar light falls on them, in which all that blue is involved; though by the predominance of the red rays, they appear of that colour, without any visible tincture of blue.

In order to determine whether the purple appearance of the red extremity of the spectrum, when viewed through a blue glass, was caused by any of the white solar light, which might perhaps be reflected from the air, or surrounding objects to the spectrum, and thus throw on that part such a quantity of blue as might produce a sensible effect; I caused the



middle and most intense part of the red to pass through a hole in a blackened paper, and then fall on an optical screen; by which I was sure that I had as pure and uncompounded a red as could be desired; which also underwent the usual test of purity by subsequent refraction, without any change in the form of the spectrum; I then looked at the body which was illuminated with this red, through the same blue glass, and the effect was the same as before.

To try this doctrine of three parent colours still farther, I considered, that if the orange were really compounded of the red and yellow rays, then by looking at the orange through a red glass, the orange would in a great measure vanish, and the red would appear to extend much farther than in the original spectrum; because the yellow rays being considerably obstructed, the red would become more predominant; and that part of the spectrum, which before appeared orange, in consequence of a certain mixture of yellow and red, would now, by the failure of so considerable a part of the yellow, lose its orange appearance, and put on that of red: and, on experiment, I found the case to be so really in fact; for while an assistant looked at the spectrum through the red glass, I moved an obstacle from the red towards the other end of the spectrum, desiring him to stop me, when the obstacle should arrive at the confines of red and orange; but when he did so, the obstacle had attained the middle of the orange; or rather had passed beyond it. Now if the orange were really a primitive colour, I should suppose, that when looked at through the red glass, it would either appear diluted, without any change of dimensions; or that if the weak part of the orange, next the red, should vanish, by the obstruction of the glass, a dark interval would appear between the orange and the red; in neither case can we account for the apparent extension of the red into the region of the orange; nor by any other hypothesis, as appears to me, than that some of the red rays are equally refrangible with some of the orange.

There is another argument derived from the ocular spectra of Dr. Darwin, which still further corroborates the doctrine of three primogenial colours. Place a piece of coloured silk, about an inch in diameter, on a sheet of white paper, about half a yard from your eyes; look steadily upon it for a minute; then remove your eyes upon another part of the white paper, and a spectrum will be seen of the form of the silk thus inspected, but of a different colour, thus

Red silk produced a green spectrum,	
Green	- red,
Orange	- blue,
Blue	- orange,
Yellow	- violet,
Violet	- yellow.

The reason of these phenomena is very ingeniously assigned by Dr. Darwin; he says, that the retina being excited into a violent and long continued action by the red rays, in the first experiment, at length is so fatigued as to become insensible to them; but that it  
still

still remains sensible, that is, liable to be excited into action by any other colours at the same time; and therefore the spectrum assumes a green appearance, because if all the red rays be taken out of the solar light, the remaining rays will compose green. See Phil. Trans. Vol. LXXVI. Conversely, a green object produces a red ocular spectrum. Now we may observe, that if all the green rays be taken out of the solar spectrum of seven colours, the remaining colours will not compound red. If indeed green be not a primitive colour, but a composition of blue and yellow, then will the eye, in looking on a green object, be at once affected by blue and yellow rays; and therefore become insensible to them both; and consequently the spectrum will appear red. But if green be a primitive, original colour, generated by its own peculiar green-making rays, the eye in contemplating a green object, will become insensible only to the green rays; and therefore the other six prismatic colours, which are specifically different from the green, ought to be sensible, and produce their proper compound effect; but this would not be the sensation of red. In like manner, if the object be yellow, the eye will at length become insensible to the yellow-making rays, and the spectrum will be violet. Now since on the hypothesis of seven original colours, the orange and green are primitive, though the eye be rendered insensible to the yellow rays, it will not be so to the orange and green, which therefore, together with the red, blue, violet and indigo, will produce their compound effect; but the colour resulting from this joint action is not violet, which nevertheless is the colour of the ocular spectrum. On the other hand, if there be but three primitive colours, red, yellow, and blue, when the eye is insensible to the yellow-making rays, the spectrum must necessarily be violet, which is the colour that results from the mixture of red and blue. If it be objected, that the eye is not only insensible to the unmixed yellow rays, but likewise to the yellow of the orange and the green, then it is admitted that orange and green are compound colours. Besides, since the colour which would result from the mixture of red, orange, green, blue, indigo and violet is not yellow, the eye ought not to be insensible to this colour; and consequently, since by the exemption of the yellow rays from the white solar light, that colour does not result, but a distinct purple, it follows, that the orange and green are not primitive colours inherent in solar light.

It remains now only for us to shew, that the three colours of red, yellow, and blue, are adequate to the solution of all the phenomena of chromatics. But in order to shew this, few words will be sufficient, for having seen, that the seven prismatic colours can be generated by these three, it follows that all others can be generated from them, as Sir I. Newton has proved at large. However, I think it will not be superfluous to observe, that white may be directly produced by these three colours, without the previous generation of the other four prismatic colours, in the same manner as it is usually generated with seven. "I could never yet," says Newton, "by mixing only two primary colours, produce a perfect white. Whether it may be composed of a mixture of three, taken at equal distances in the circumference, I do not know." Now to shew that white may be thus generated, let an annulus of about four inches diameter be divided into three parts by lines tending towards



towards the centre, and let these three divisions be respectively painted red, yellow, and blue, in proportions to be ascertained by trial; then if the annulus be turned swiftly round its centre, it will appear white. That white may be generated by the mixture of only the three colours red, yellow, and blue, might also appear from the rule which Newton himself has given us, for determining the colour of the compound which results from the mixture of any primary colours, the quantity and quality of each being given.

The rule is this, the circumference of a circle is distinguished into seven arches proportional to the seven musical intervals in an octave, that is, proportional to the numbers 45, 27, 48, 60, 60, 40, 80: the first part is to represent a red colour, the second orange, the third yellow, the fourth green, the fifth blue, the sixth indigo, and the seventh violet. These are to be considered to be all the colours of uncompounded light gradually passing into one another, as they do when made by prisms, the circumference representing the whole series of colours from one end of the sun's coloured image to the other. Round the centers of gravity of these arches let circles proportional to the number of rays of each colour in the given mixture be described. Find the common centre of gravity of all these circles, and if this common centre of gravity coincide with the centre of the circle, Newton says that the compound will be white. Join therefore the centers of gravity of the blue and yellow circles, and from the centre of the red circle draw a right line through the centre of the principal circle; from the construction it will cut the line which joins the centers of the blue and yellow circles; if therefore the number of the blue and yellow rays be to each other inversely as their distances from the point where the line which joins their centers is cut by the line drawn from the centre of the red circle; and if the number of red rays be to the sum of the yellow and blue rays inversely as the distances of the centre of the red circle, and the common centre of the yellow and blue from the centre of the principal circle, the common centre of gravity of the red, blue and yellow circles will coincide with the centre of the principal circle, and therefore the resulting compound will be white.

But it is manifest that this construction cannot be relied on, because the quantities of the rays of any given colour in solar light, do not appear to be proportional to the spaces which they occupy in the rectilineal sides of the spectrum. Thus it is known that the yellow making rays are predominant in solar light, yet the space they occupy in the spectrum is to the space occupied either by green or blue as four to five, and to the space occupied by the violet only as three to five.

## II.

*An Account of some Additional Experiments and Observations on the Galvanic Phenomena.*

*By Mr. DAVY, Superintendant of the Pneumatic Institution. Communicated by the Author.*

## I.

*Additional Experiments on the Causes of the Galvanic Phenomena.*

1. **S**ULPHURIC acid, when highly concentrated, is possessed of but little power of action upon zinc, though when diluted it dissolves it with the greatest rapidity. Assuming then the truth of the principles advanced in my last paper, namely, that the powers of the pile of Volta are primarily excited by the oxidation of the zinc, it follows, that diluted sulphuric acid, when made the medium of connection between the pairs of plates, ought to produce much greater effects than concentrated sulphuric acid.

This I have found is actually the case. When the cells of a series of twenty pairs of plates of silver and zinc, constructed with waxen cement, in the mode described in 8 of my last paper, were filled with sulphuric acid, nearly of specific gravity 1.9, no galvanic action, except the production of a slight caustic taste, was perceptible by the usual methods of trial; though when diluted sulphuric acid was used the ends of the series gave shocks to the moistened fingers, and wires connected with them effected the usual changes in water.

That concentrated sulphuric acid is not of that order of more perfect galvanic conductors which, when interposed between the plates, destroy their electrical effects, is evident from the following experiment: the cells of ten pairs of plates of copper and zinc, constructed with waxen cement, were filled with concentrated sulphuric acid; but not the slightest galvanic power was produced. A small drop of water was then poured upon the acid in each of the cells. The action of the series was immediately shewn by its producing the usual appearances on wires in water.

2. The galvanic conducting powers of liquid sulphurets are at least equal to those of water. I found that when the fingers were plunged into glasses, containing solution of sulphuret of strontian connected with the ends of a pile, the shock was full as sensible as if the communication had been made through water. When the galvanic current was made to act on solution of sulphuret of strontian by means of silver wires, the zinc wire became blackened, and gas was given out round the silver wire. But solutions of sulphurets are incapable of giving oxygen to zinc; they, consequently, ought to produce no galvanic effects when made the media of connexion of the double plates in the pile of Volta. Twenty-five pairs of silver and zinc, erected with cloths moistened in solution of sulphuret



phuret of strontian, produced no sensible galvanic action, though the moistened the sides of the pile were moistened with a little nitrous acid, the ends gave shocks as powerful as those of a similar common pile.

3. No phenomenon is more constant than the cessation of the action of the common galvanic pile in a vacuum\* when the gage is below one-tenth. Supposing the expulsion of atmospheric air from the water preventing it from oxidating the zinc, the sole cause of this cessation, it follows, that a pile ought to act in vacuo when nitrous acid, or diluted sulphuric acid, is the medium of connexion between the plates. Into each of the cells of a series of twelve pairs of silver and zinc plates, which had just been moistened with a little water, a large drop of nitric acid was introduced: when the wires connected with the ends immediately began to produce the usual appearances in water. The series was introduced under the receiver of an air pump, and the silver wire from its zinc end connected with a vessel of water that had been long boiled. The wire from its silver end was so fastened by resinous cement, to a sliding brass wire passing through the top of the receiver, that it could be plunged at pleasure into the water when the vacuum was made. The receiver was exhausted till the gage stood at  $\frac{6}{10}$  of an inch, when the communication was effected. The zinc wire immediately began to oxidate, and gas was given out round the silver wire. The process went on for many minutes, and when it had ceased, was not sensibly revived by the admission of the atmosphere. In another experiment the same phenomena were observed. Gas appeared to be given out more rapidly from the silver wire than in the atmosphere; but this was from the diminution of pressure. The oxidation was certainly less: which may be easily accounted for, when we consider, that no nitrous acid could be recomposed in vacuo, as in the atmosphere from the nitrous gas disengaged between the plates, and that, in consequence of the diminished pressure, some of the acid must probably have assumed the æriform state.

A drop of sulphuric acid, poured into each of the moistened cells of twelve pairs of plates, enabled the wires from the ends to effect the usual changes in pure water for rather more than half an hour in vacuo, the gage being at  $\frac{6}{10}$ . The oxidation went on nearly as vividly as in the atmosphere, and what is rather remarkable, some gas was given out from the oxidating wire, though very little was produced from the silver wire.

4. The results of the last experiment are interesting not only from their coincidence with the other facts, but likewise because they afford proofs that the presence of oxygen in that loosely combined or peculiar state in which, when absorbed by combustible bodies, it produces inflammation, and in which, in my infant chemical speculations, I supposed it to be combined with the matter of light, is not essential to the galvanic effects. Whether water is absolutely essential, we shall find some difficulty in determining, as it exists in larger or smaller quantities in all the non-metallic fluid galvanic conductors that have been yet experimented upon. The following fact is in favour of

\* See Colonel Haldane, Phil. Journal, No. 43, Vol. IV. and my last paper, No. 44.

its essentiality. The compound of concentrated sulphuric acid and oxygenated muriatic acid \* (which may be formed by introducing oxygenated muriate of pot-ash into sulphuric acid, or by passing oxygenated muriatic acid gas through it), slowly oxidates both zinc and silver, the oxygenated marine acid being decomposed. I expected that it would produce strong galvanic effects when made the communicating medium of the cells of a pile; but in this I was disappointed; a series of twenty pairs connected by it produced hardly any sensible action.

5. If any person wishes to repeat the experiments in vacuo just detailed, great caution must be observed with regard to the quantities of acids introduced into the cells. Two or three drops in each will be sufficient, particularly if it has been previously a little moistened. When larger portions are employed, the effervescence highly increased by the removal of atmospheric pressure will be often sufficient to moisten the edges of the cells, and to make a communication between them. In consequence of the use of too much acid, I have made many unsuccessful experiments.

The water used in vacuo for connecting the wires should be always previously deprived of loose air by long ebullition, or otherwise the disengagement of that substance from it will much disturb the results.

## II.

### *Observations gained from minute Inspection of Galvanic Processes.*

1. Whenever the galvanic circuit, passing through the pile with wires, is broken by means of water, oxygen is uniformly produced at the zinc metallic point, and hydrogen at the silver metallic point. This is shewn from many experiments in Mr. Nicholson's Philosophical Journal. Considering analogies, an interesting question occurs. Do not the same phenomena take place in every part of the series? *i. e.* is not oxygen fixed on every plate of zinc, and hydrogen produced on every plate of silver, at the points of their contact with the water of the cloths? With the hopes of gaining a solution of this question, I constructed a series of twenty glasses with spring water, containing plates of silver and zinc connected by brass wire, in the mode pointed out by Volta. This series gave feeble shocks, and a silver wire connected with it produced the usual appearances in water. Oxygen was fixed upon that part of the wire in the glass containing the last silver plate, and hydrogen was liberated from that part of it in the glass containing the last zinc plate.

The series was made analogous in all its parts, the end glasses being connected by a pair of plates, so that every glass contained a silver plate and a zinc plate. On minutely inspecting the glasses immediately after, no particular phenomena could be perceived. But after occasional attention to the process for many hours, I observed the zinc plates

\* I accidentally discovered this combination in July, 1799. Some of its properties are very peculiar. More interesting enquiries have hitherto prevented me from minutely examining them.



beginning to oxidate in many of their points, though no gas was produced upon them. No gas had formed upon the silver plates, but the surface of the water in contact with them became covered with an opaque white pellicle.

2. The silver plates used in this experiment were not perfectly polished. This might have influenced the results: and some gas might have escaped my observation. That the operation might go on in closed vessels, I cut off the bottoms of some bottles with a file, so that they could be easily joined again by cement. Into one of these bottles I introduced a plate of polished zinc\*, and into another a plate of polished silver. The plates were connected by a wire attached to their upper angles, which protruded into the atmosphere through orifices made at the places of junction of the bottoms of the phials with their sides; these places of junction being rendered perfectly water-tight by resinous cement. Four apparatuses of this kind were constructed. They were filled with pump water, inverted in the galvanic order in glasses containing that fluid, and made part of a connected series of twenty glasses.

After more than twelve hours, the zinc plates had become tarnished, but had given out no gas. In two of the bottles with the silver plates, globules of gas, too small to be analysed, had collected: these plates examined in the atmosphere, as well as all the other silver plates, were covered in some points with a film of white substance, which was soluble with slight effervescence, and without producing cloudiness in muriatic acid.

3. Unable to account for the non-appearance of hydrogen during the oxidation of the zinc, I could not but conclude that it was condensed or absorbed in some new compound on the surface of the silver or the zinc. Guessing that the quantity of surface might be connected with the phenomenon of its non-appearance, I substituted in three of the phials for the square silver plates oblong ones of the same length, and about .3 inches wide. These had not been long connected with the series before gas began to form upon them; and in five hours sufficient was collected to be examined: from the coarse test of inflammability it appeared to be hydrogen.

Thirteen pairs of a connected series of twenty-five glasses were now composed of square zinc plates, and oblong silver plates of different sizes; some of them being about .3, and others not more than .1 inches wide. Gas was almost immediately given out from the greater number of the oblong plates, and in largest quantities from the smallest: from the slips of .1 wide indeed a constant stream of globules ascended through the water.

Small oval, circular, and square plates of nearly equal surfaces, with the slips connected in the series in the places of some of them, produced precisely the same effects. In short, whenever the surfaces of the silver plates did not exceed one-fourth of the quan-

\* Of 1.2 inches square, the size used in all the former experiments.

tity of the surfaces of the zinc plates, whatever were their forms, gas was always produced upon them: and both large and small surfaces in common water in a great length of time became covered at some of their points of contact with that fluid with a whitish film.

When small oblong zinc plates were introduced into any part of the series instead of the larger plates, they appeared to oxidate rapidly, without giving out any gas.

4. The substitution of oblong silver slips for many of the plates, did not apparently much diminish the power of the series: I therefore constructed a series of twenty-seven glasses, wholly composed of zinc plates attached to thin silver wires. This combination with pump water gave feeble shocks, which were less vivid than those of the common series of eighteen. When, however, it was made analogous in all its parts, all the wires not deeply inserted in the water gave out gas, and the zinc plates slowly oxidated. In another experiment, in which a series of thirty glasses, containing wires and zinc plates, were used, most of the wires not only gave out gas, but after some time became covered at their points of contact with the surface of the water with a white film; a few of them, not deeply inserted, produced a slight white precipitation.

5. These facts seemed to shew that the quantity of hydrogen produced in a series was in some measure, and to a certain point, in the inverse ratio of the quantity of the surface of the silver plates. Speculating upon them, and comparing them with the experiments of Mr. Cruickshank, and those which I noticed in my last paper on the signs of ammoniac perceived during the action of a pile in common air, I could not but conjecture that whilst oxygen was condensed on all the zinc excitors in the series, hydrogen was produced on all the silver ones; and in small surfaces chiefly liberated, whilst on larger ones it was almost wholly condensed by the nitrogen of atmospheric air dissolved in the water, and this conjecture was rendered more probable, when I considered the white matter chiefly formed round the silver at the surface of the water, and its solubility, without cloudiness in acids, as it might easily have been produced by the decomposition of magnesian salts existing in the pump water.

To determine whether ammoniac was produced, I made many experiments on different series, consisting of from seventeen to thirty glasses. In some of these glasses wires were used, and in some of them plates. Sometimes distilled water was employed, and sometimes pump water, both of which were occasionally tinged with red cabbage juice.

Without being minute in the detail of these experiments, I shall give their general results. In the vessels containing red cabbage juice, that fluid, after many hours, became tinged with green where it was in contact with the silver, though at its points of contact with the zinc no change of colour could be observed in it. In the pump water a white film always formed on the surface of the water near its points of contact with the silver: whilst in distilled water such an appearance was hardly perceptible. The anomaly of its being now and then perceptible I am inclined to refer to accidental impurities in the vessels.

The



The silver slips in pump water almost always became incrustated with a white matter, which was never notable in distilled water, and which was soluble, without cloudiness, in nitrous acid.

In one experiment, when a silver slip, forming part of a powerful series, was introduced into a small vessel, containing solution of muriate of magnesia\* (connected with the next zinc glass by means of muscular fibre, to prevent the interference of the oxydating metal with the results), in the course of a night much gas was given out from it, and it became incrustated with a white matter, which dissolved with slight effervescence in marine acid. A precipitation had taken place in the fluid.

These results afford strong probabilities in favour of the production of ammoniac on all the silver exciters of the series formed with common water: and compared with the facts before mentioned, they amount almost to demonstrations. Whether the nitrogen of atmospheric air dissolved in water is the agent which forms with the hydrogen ammoniac, future experiments made in vacuo must determine †.

6. The power of the series with silver wires, was much less than that of the series with plates. Supposing the formation of ammoniac, it was probable, that the larger quantity produced upon the plates, might be in some measure the cause of their greater powers; and if so, it was likely that the condensation of nascent hydrogen upon the wires would be connected with increase of power. From the following facts it appears that this is the case. A series of thirteen plates of zinc, with their silver wires constructed in glasses, containing weak solutions of red sulphate of iron, mingled with a little solution of common sulphate of iron and nitrous gas, acted full as powerfully as the common series of twenty plates. The wires gave out no hydrogen, but occasioned a brown precipitation in the fluid, and the zinc plates soon became covered with green oxide of iron. The phenomenon was the same with other mixed metallic solutions, capable at the same time of absorbing hydrogen and oxydating zinc.

7. The strongest analogies would induce us to believe, that all the galvanic series composed of easily oxydable metals and difficultly oxydable metals must follow the same laws in producing changes in their connecting fluid as zinc and silver. But as from the interesting facts of Colonel Haldane, it appeared that iron and zinc as a combination were possessed of very considerable powers, and as iron is but little inferior to zinc in its affinity for oxygen,

\* Muriate of magnesia was used in preference to nitrate or sulphate, because there was a possibility of the decomposition of the acids in these salts by the nascent hydrogen, which alone would occasion a precipitation of earth.

† Mr. Cruickshank, who first noticed the probable formation of ammoniac on the silver wire of the pile, has offered some ingenious arguments to prove that nitrous acid is formed at the zinc wire. Analogy would induce us to conjecture, that if it was formed on one of the oxydating surfaces, it ought to be formed on all. That no change of colour takes place in cabbage juice, in contact with the oxydating zinc plates, may be owing to the great extension of their surfaces. It is worth observing, that the silver oxydates as rapidly in water in vacuo as in the atmosphere, when the pile is in the atmosphere; or when nitrous or sulphuric acid is used in the cells in vacuo.

it became a curious question what would be the habitudes of those metals in effecting changes in the water constituting them a series? Sixteen zinc plates, and sixteen pieces of thin polished iron wire, were connected in galvanic order. One half of the series of glasses being filled with solution of red sulphate of iron, and the other half with pump water, the end glasses gave faint shocks when the tongue was introduced into one, and the fingers into the other. When they were connected so as to make the series analogous in all its parts, all the iron wires in the common water gave out gas without oxydating, and these in the solution exhibited the same appearances as the silver wires in II. 6.

8. A series of glasses composed of zinc plates and silver wires in galvanic order with pump water, was suffered to remain for some hours without being connected at the extremities, so as to complete the circuit. At the end of this time some globules of gas appeared on some of the zinc plates, which were a little tarnished; no gas was produced on the silver wires, and they had undergone no apparent change.

Having set up a powerful series of 27 glasses, (some with red sulphate of iron, and some water) in which zinc and silver wire were the excitors, I found that whenever I supplied the place of a pair of plates, either by a single metallic wire, or a chain composed of different metals, whatever were their habitudes of oxydation, hydrogen was always produced at the place of the silver, and oxygen always fixed or extricated at the place of the zinc. When many silver wires were introduced into the series in new glasses, so as to preserve the original number of exciting plates, the powers of it seemed to be very little diminished, and gas was given out, and oxygen fixed in every new glass. When the points of contact of some of the plates and wires above the water were covered with cement, the phenomena were the same as if they had been exposed to the atmosphere. When one pair of a series was in vacuo, the gage being at  $\frac{6}{10}$ , the powers of the whole were not sensibly diminished, and gas was extricated from the silver wire.

9. On these facts I shall not presume to speculate. There is every reason to believe that a number of new experiments must be made, before we shall be able to discover the laws in consequence of which one quantity of chemical action generates in the galvanic series of Volta, an influence capable of increasing all analogous actions, and of generating new similar actions. Many new observations must be collected, probably before we shall be able to ascertain whether water is decomposed in galvanic processes. Supposing its decomposition, we must assume, that at least one of its elements is capable of rapidly passing in an invisible form through metallic substances, or through water and many connected organic bodies; and such an assumption is incommensurable with all known facts. But a short period is elapsed since philosophers beheld with wonder, solid and fluid substances assuming new modes of existence in different gases. Do not the new phenomena of galvanism authorise us to hope that at no very distant time they will behold even those gases undergoing novel changes, and existing in new and now unknown forms?



## III.

*Remarks on the Powers of different Galvanic Combinations.*

1. I have found by many experiments, that when muriatic acid is introduced (in quantities sufficient to produce visible changes) into water contained in the glasses of an effective galvanic series composed of zinc plates and silver wires, the zinc plates are acted upon, and gas is given out from every part of their surfaces; whilst the quantities of gas produced on the silver wires are increased only in the apparent ratio of the increased power of the series to give the shock. When muriatic acid is introduced into a certain number only of the glasses of a series, similar changes are produced in those glasses; but there is no apparent alteration in the nature of the phenomena taking place in the other glasses; these phenomena are only rendered more vivid. Effects analogous in *appearance* take place when sulphuric acid, and even when nitric acid is employed. So that it appears that the power of a series to fix oxygen upon its zinc plates, and to evolve hydrogen from its silver plates, is limited; and cannot be increased by oxydating bodies beyond a certain extent. We must consequently conclude, that only a certain quantity of galvanic influence can be made to circulate through a series in a given time, and that the increase of oxidation beyond a certain term is connected with no new increase of power.

2: The substances which are capable of rapidly oxydating the imperfect metals, and of condensing nascent hydrogen at the same time, are those which produce the most powerful effects, when made the media of connection between the metals in the galvanic series. The nitric and oxygenated marine acids appear to be the most powerful of the known fluid exciters of the pile. The solutions of metallic salts, composed of acids and oxydes at their maximum of oxidation, stand next in order: then follow the muriatic and sulphuric acids, and the neutral salts containing those acids, or nitric acid.

I have lately endeavoured to make some comparisons between the powers of piles constructed with nitric acid, and those of common piles; but without much success. The rapid action of the acid, the evolution of gases connecting the plates by moisture, and the production of heat, all tended to disturb the results. The smallest series of plates from which I was able to obtain shocks by nitric acid, was composed of three pairs. Six pairs, with moderately strong nitric acid, gave a shock more acute than that produced by a common pile of twenty-seven pairs, but apparently much more limited in extent: it was felt no further than the upper joints of the fingers. With twelve pairs, or still more numerous combinations, the shock was always more acute and painful, than from a common pile composed of four or five times the number of plates; but apparently more local, and felt over a smaller surface. As fusion of the cement always took place when the trough was used in these experiments, cloths wetted with the acid were generally employed; but in no case was it possible to prevent the edges of the plates from being moistened, so that a certain degree of communication between the ends always existed, and from this communication the effects must have been much diminished. A pile with nitric acid had its power

very little lessened by momentary immersion in water. Hence the increase of effect cannot much depend on the increased temperature of the plates.

3. Copper and zinc act very powerfully with nitric acid. Iron and zinc seem to act nearly as intensely with muriatic acid as with nitric acid.

The galvanic combinations that I have been lately most in the habits of using, are series of glasses constructed with zinc plates, silver or iron wire, and solutions of red sulphate or muriate of iron. These combinations act for a long time intensely; six or eight glasses being capable of slowly decomposing water; and if their action is at any time diminished from the deposition of oxide of iron on the zinc plates, it may be easily restored by the addition of a little acid to the solution.

4. I noticed in a former paper the conducting powers of charcoal, when made part of the galvanic circuit\*, I lately set up a series of eight glasses, with small pieces of well burnt charcoal, zinc, and solution of red sulphate of iron; the charcoal and zinc being connected by silver wire. This series gave sensible shocks, and rapidly evolved gases from water; whilst an equal series with silver and zinc produced much weaker effects. Hence it would appear that charcoal and zinc are equal, if not superior, to any metallic combinations.

\* \* In my last paper two errors occur, which require to be corrected. Page 340, line 18, for  $\frac{6}{40}$  read  $\frac{6}{10}$ ; and page 341, line 11, for *proportional* read *in great measure proportional*.

### III.

*On the Quotients arising from the Division of an Unit by prime Numbers. - By*  
H. GOODWYN, Esq.

TO MR. NICHOLSON.

SIR,

THE following account of the quotients arising from the division of an unit, &c. by prime numbers, being, I believe, perfectly new, and promising to be very useful, is very much at your service; and if you think it worthy a place in your Journal, it may induce the publication of a small table prepared for a farther elucidation of the subject.

I am, SIR,

Respectfully yours,

East-Smithfield, Oct. 1800.

H. GOODWYN.

\* Dr. Wells, in an excellent paper on galvanism, in the Phil. Transf. has mentioned the great powers of an arc composed of charcoal and zinc, in exciting the limbs of frogs.

The



The quotient of an unit, divided by the prime number 17, will consist of 16 places of figures, forming a complete circulating decimal. If the numbers, 2, 3, 4, &c. to 16, be divided by the same prime number, each respective quotient will still consist of 16 places of circulating decimals. Thus far the property of like divisions has been ascertained by various writers on decimal arithmetic, &c.

But at least one very curious, concise, and useful property attached to similar divisions in general, yet remains to be unfolded. It is this—that the quotient arising from the first division, virtually represents the quotient of every other division above-mentioned.

### FIRST QUOTIENT.

Dividends.	Divisors.	
For 1	÷ 17	= 0 5 8 8 2 3 5 2 9 4 1 1 7 6 4 7
2	÷ 17	= { 0 5 8 8 2 3 5 2 9 4 1 1 7 6 4 7
3	÷ 17	= { 0 5 8 8 2 3 5 2 9 4 1 1 7 6 4 7
4	÷ 17	= { 0 5 8 8 2 3 5 2 9 4 1 1 7 6 4 7

And in like manner the quotient arising from the division of an unit by every other whole number, less than the divisor, will commence with a different figure in the first quotient, and will circulate to that figure again.

And thus the complete quotient arising from the division of each whole number, less than the divisor, in that division may be expressed on the first quotient, by placing the respective dividends over their first quotient figure.

Thus { 1 10 15 14 4 6 9 5 16 7 2 3 13 11 8 12 dividends.  
0 5 8 8 2 3 5 2 9 4 1 1 7 6 4 7 quotients.

This disposition of the dividend and first quotient enables us to find, by inspection, the complete decimal quotient or expression for a vulgar fraction, whose numerator or dividend is any given whole number, between 1 and 17, and whose denominator or divisor is 17. The quotient of  $\frac{2}{17}$ ,  $\frac{3}{17}$ , and  $\frac{4}{17}$ , is seen in the first elucidating arrangement above, and perfectly coincides with this last. By this, if I want the complete decimal quotient of  $\frac{16}{17}$ , I have only to search for the number 16 in the line of dividends, and under it is the first figure of the circulating decimal that will comprize complete quotient of  $\frac{16}{17}$ , viz. .9411764705882352, and the same of the other dividends.

But what authorises the above property to be termed curious, concise, and useful, is, that it does not attach to the prime number 17 only, but under certain laws is equally applicable to all prime and multiples of prime numbers whatever.

## IV.

*Construction of a Wheel adapted to express by its Rotation the unequal angular Motion of the Planets. By M. ROEMER.*

TO MR. NICHOLSON.

SIR,

AS you have professed it to be part of your plan to insert all discoveries in your Journal without regard to date, provided they possess sufficient merit, and are not sufficiently known in this country, I take the liberty of recommending to your notice an invention of singular ingenuity, upwards of a century old, but so little known, that the (in my opinion inferior, though happy) contrivance of Desaguliers was offered to the public forty years afterwards, and still continues in use.

I am, SIR,

Your's, &c.

R. B.

If it be desired \* to move a wheel of 24 teeth by a pinion of six, in such a manner that in certain parts of its revolutions it shall move so swiftly as if it had but 12 teeth, and in other parts as slowly as if it had 48 teeth, the method of accomplishing this is as follows :

1. Describe the right angled parallelogram  $L M N O$ , Fig. 1, Plate XVII. having its side  $N O$  equal to the diameters of the great wheel and the pinion taken together, and its breadth  $L N$  equal to their thickness, which last must be greater, the more considerable the inequality of the intended movement.

Divide  $N O$  in  $Q$ , in such a manner that  $Q O$  may be to  $Q N$  as 6 to 48, that is to say, reciprocally as the velocity of the pinion to the greatest velocity of the wheel.

Divide also  $L M$  in  $P$ , in the proportion of 6 to 12, or reciprocally as the velocity of the pinion to the smallest velocity of the wheel. Then draw  $P Q$ , and as many lines  $S R$ , parallel to  $L M$ , as there are teeth intended in the great wheel, upon which write the degree of velocity they express, which are in the inverted ratio of their lengths.

2. Let two truncated cones be made in the lathe; one equal to what might be formed by the revolution of the trapezium  $L P Q M$  round  $L N$  as an axis, and the other equal to what might be formed by the revolution of the trapezium  $P Q M O$  round the axis  $M O$ .

\* In what follows I translate and take the figure from the *Machines et Inventions approuvées par l'Académie Royale des Sciences* for 1699, page 89.---B.



On the largest of these two cones let the circles generated by the revolution of the points P, T, Q, be marked and distinguished by the same numerical figures as the corresponding parallels of the parallelogram L O.

Describe upon the two bases of the conical frustum radial lines, which shall make angles at the centre C, Fig. 3, in the same proportion to each other as the intended velocities of the wheel, as expressed in Fig. 2, and let teeth be cut in the surface of the cone according with these lines; after which look on the circles which express the different velocities, and have been traced on the same surface, to find what part of each tooth ought to remain opposite its corresponding radius, and cut or file away the rest. The teeth will thus lie in an elliptical or inclined curve on the conical face, which in the figure is marked by a darker shade.

The pinion must be made of a regular conical form, as is shewn at M O, in Fig. 3.

By this contrivance the largest or widest teeth will always meet the largest part of the pinion, and the narrowest will correspond with the smallest part; by which means, though the pinion has an uniform motion, the wheel will be carried unequally, according to the required law \*.

## V.

*On the Solutions and Precipitates of Mercury. By Cit. BERTHOLLET†.*

WHITE sulphate of mercury, slightly oxidized, as described by Citizen Fourcroy in the last volume of the Academy of Sciences, is obtained with more ease, and in a purer state, by boiling very diluted sulphuric acid upon mercury. When the liquid undergoes a strong heat, it becomes oxygenated sulphate of mercury. When there is not too great an excess of acid, it is in part decomposed by the water, which becomes acid by seizing a portion of the acid. If, however, contains less acid than the mild sulphate. The proportions between the precipitated oxidized sulphate, and the acid sulphate suspended in the water, are different, according to the temperatures, the quantity of water, &c.

When the sulphate is decomposed by an alkali, the precipitated oxide always retains a small quantity of acid.

Nitric acid has the same habitudes with the oxides of mercury. Citizen Gay has remarked, that when mercury is dissolved by heat in nitric acid, there is first a disengage-

\* The contrivance of *Desaguliers*, described in his *Course of Experimental Philosophy*, I. 464, consists of two elliptical wheels, Fig. 4 and 5, connected by tooth or by catgut (which last is a very bad way.) They revolve on their foci, and while the driving ellipsis moves uniformly, the radius vector of the other has the required motion.---R. B.

† Société Philomath. No. 41.

ment of nitrous gas; after which solution then takes place quietly, the oxygenated nitrate, which was first formed, being decomposed by the portion of mercury which remains. The nitric solution of mercury made by heat, cannot keep all the oxide of mercury in solution, except by means of an excess of acid.

Muriate of soda precipitates the nitric solutions of mercury, and affords muriates which differ according to the degrees of oxygenation of the mercury in its solutions.

Though the oxides of mercury, when too much oxygenated, cannot remain combined with the sulphuric and nitric acids, it is not the case with respect to the muriatic acid which, not being saturated with oxygen like the two former dissolves mercury, and the other metals at every degree of oxidation. Thus mercury combined with that acid in the oxygenated muriate, is much more oxidized than in the most highly oxidized of its nitric combinations.

Bayen has proved, that most of the precipitates of mercury contain a small quantity of acid. Citizen Berthollet has added new experiments to those already made by this chemist.

When a muriatic solution of mercury is precipitated by carbonate of soda, an examination of the precipitate, and of the supernatant fluid, proves that the latter contains the alkali with a great part of the carbonic acid, muriatic acid, and a small quantity of oxide of mercury. The precipitate is formed of oxide of mercury, muriate of mercury, and carbonate of mercury. When carbonate of pot-ash is used, the precipitate seizes all the carbonic acid, and a greater portion of the muriatic acid; so that it sublimes almost totally in mercurial muriate. The precipitate contains less muriatic acid, if the carbonate of pot-ash which is used contains pure pot-ash.

Bayen had also remarked, that certain precipitates of mercury possessed the property of detonating when mixed with sulphur, but he has not explained this phenomenon, and the circumstances under which it presented itself. Citizen Berthollet has shewn, that sulphur in contact with the oxides of mercury, suddenly takes from them the slightly adhering oxygen, when these precipitates contain only a small quantity of muriatic acid; but this effect cannot take place when the oxide of mercury is defended from the action of the sulphur by too large a quantity of muriatic acid.



## VI.

*A Chemical Analysis of Three Species of Whinstone, and Two of Lava.* By ROBERT KENNEDY, M. D. F. R. S. & F. A. S. Edin.\*

ON the 5th of August (1798) I announced to the Society that I had discovered soda in several varieties of the whinstone † of Scotland, and also in lava from Mount Ætna; but did not describe the various experiments to which these substances had been subjected in my examination of them. In the following paper, therefore, I have the honour of laying an account of these experiments before the Society.

## ANALYSIS I.

*Basalt of Staffa.*

The specimen of this basalt, submitted to the following analysis, was given me by a gentleman, who brought it himself from the celebrated basaltic columns in Staffa. A description of its external mineralogical characters may be found in Sir James Hall's paper, (p. 8. and 56 of our present Volume) to which I beg leave to refer.

This basalt, though reduced to fine powder, does not effervesce with acids. The colour of the powder is greyish, and when wet greenish. By being exposed to a low red heat, the colour of the stone is changed to brown. It is not attracted by the magnet, either in its natural state, or after being heated red hot.

Its specific gravity, taken in distilled water at the temperature of 60° of Fahrenheit, I found to be 2.872.

Some small pieces being exposed to a low red heat for half an hour, lost 5 per cent. in weight; and when the stone was reduced to powder, and heated red hot, the loss was the same. I also examined the effects of high heat on it, in the following manner: having made some small crucibles of the porcelain clay of Cornwall, which I used on account of its great purity and infusibility, I baked them in pretty strong fires, generally above 100 of Wedgwood. As soon as they were cold, they were each exactly weighed. A portion of the basalt in fragments, also weighed, being put into one of these small crucibles, and a pyrometer into another of the same size, both were placed in a Hessian crucible. A small flat cover, also made of the porcelain clay, was laid upon each; and then a lid was carefully luted on the Hessian crucible with clay and sand. The apparatus thus prepared was next set into a furnace; and the fire being raised gradually till it appeared to have attained the pitch desired, it was kept as equal as possible for about an hour. The small crucible,

\* From the Edinburgh Transactions, 1799.

† The name whinstone is used throughout this paper in a generic sense, comprehending basalt, trap, certain kinds of porphyry, wacken, and some other stones of the argillaceous class:

and the melted basalt it contained, being weighed as soon as cold, it was easy to determine how much weight was lost.

In this manner some of the basalt was exposed to a heat of 72 of Wedgwood, at which it was vitrified, and lost exactly the same weight as in a low red heat. At 160 the effects were in every respect the same; the loss not being greater in that intense fire. The small crucibles, in which the pyrometers had been placed, did not in these experiments lose the smallest weight.

The volatile matter thus driven off by heat is partly water, as the following experiment shows: I put half a pound troy of the basalt in fragments, into a small Wedgwood retort, and luted to it a receiver, into an aperture of which was fitted one end of a glass tube, the other end being adapted to a pneumatic apparatus. The retort was then heated slowly to redness, and kept moderately red hot for two hours. In the receiver some water was condensed. Some gas also passed over; but I could not ascertain with precision either its quantity or its nature, as it was mixed with the air of the receiver. I have not made farther experiments on the volatile matter contained in whins; but it deserves to be examined with attention.

This basalt being exposed to heat in a muffle, was found to soften at 38 of Wedgwood\*. Some of it being reduced to fine powder, was boiled in thirty times its weight of water for half an hour. After filtration the water was examined with different chemical tests, but gave no precipitate with any, except a slight cloud with nitrate of silver; and a portion being evaporated to dryness, left only some thin streaks on the bottom of the glass.

Having premised these particulars, I proceed to describe the analysis:

1. One hundred grains of the basalt, reduced to fine powder in a Wedgwood mortar, were mixed, in a small retort, with about 1200 grains of muriatic acid; and a receiver being adapted, the mixture was gradually heated till it boiled. It was at first of a brownish-yellow colour, but afterwards became brownish. Part of the powder was dissolved. To distill off the uncombined acid, the heat was continued till the mixture began to grow thick. It was then diluted with boiling distilled water, and poured on a filter; and the undissolved part, after properedulcoration, being dried and heated red hot a few minutes, weighed 67 grains, and was greyish-white.

2. The filtered solution was of a faint yellowish-brown colour. Being saturated with caustic ammonia, a bulky precipitate was thrown down, which was carefully separated by filtration. It had at first a dirty greenish-colour, which was afterwards changed to brown by the action of the air.

\* The fusibility of this, and the other substances to be afterwards mentioned, I examined with Sir James Hall. For this purpose, a small piece of each was placed, with a pyrometer as near to it as possible, in an open muffle previously heated to redness. It could thus be seen perfectly during the operation, and the fire being raised, as soon as it was found to be soft, when pressed slightly by an iron rod, the degree of heat was ascertained by measuring the pyrometer.

† The mortar I used was not scratched by any of the whins or lavas mentioned in this paper.



3. The solution, after being freed from this precipitate, was perfectly colourless and transparent. I dropt into it a small quantity of sulphuric acid, which produced no cloud; consequently the solution contained neither barytes nor strontian. It was then evaporated to a small quantity, and treated with carbonate of ammonia. Some white earth was thrown down, apparently carbonate of lime, which after being washed, dried, and heated red hot a few minutes, weighed  $6\frac{1}{2}$  grains.

4. The insoluble residuum, No. 1. which weighed  $67\frac{1}{2}$  grains, I mixed in a silver crucible with a solution of caustic pot-ash, containing as much alkali as was equal to twice the weight of the residuum. This mixture, being evaporated to dryness, was exposed for one hour to a red heat, in which it melted. When cold, the mass was green. After being softened, and washed out of the crucible with boiling distilled water, it was super-saturated with muriatic acid, by which the greater part was dissolved. This mixture, being then evaporated to a small quantity, became gelatinous. It was next diluted with water, digested, and filtered. Some filix remained on the filter, which, after proper washing being dried, and heated red hot a quarter of an hour, weighed 43 grains, and was perfectly white. To learn whether this filix was free from every other earth, I mixed a part of it with four parts of carbonate of soda, and melted the mixture in a silver crucible. Water, being poured on the melted mass, dissolved it entirely into a liquor silicum, which was diluted largely, and saturated exactly with an acid. No precipitate appeared, even after six or eight days; therefore these 43 grains were pure filix.

5. The solution, No. 4. (from which the filix had been separated), was of a light greenish colour. Caustic ammonia, when poured into it, threw down a brownish precipitate. Having carefully separated this precipitate, and washed it on a filter, I dropt into the remaining solution, which was now colourless and transparent, a small quantity of sulphuric acid, in order to detect barytes or strontian. No precipitate was formed. The solution was then evaporated to a small quantity, and treated with carbonate of ammonia, by which a second portion of carbonate of lime was obtained. Its weight, after being heated red hot, was  $9\frac{1}{2}$  grains.

6. The brownish precipitates, thrown down from the solutions No. 2. and 5. by caustic ammonia, had the appearance of argil mixed with iron. To separate the argil, these precipitates were mixed together, and boiled, while still moist, in a solution of caustic pot-ash, in a silver crucible. A part was dissolved; but a spongy matter remained, of a darker brown colour than at first, which was collected on a filter.

7. Into the caustic-alkaline solution I poured sulphuric acid, till slightly in excess, and neutralised it again by carbonate of soda. The argil was precipitated; which being sufficiently washed, was re-dissolved in diluted sulphuric acid. This solution was then mixed with some acetite of pot-ash, and gave, by successive evaporations, small regular crystals of alum. At last it became gelatinous; and being evaporated to dryness, and diluted again with water, one grain of filix was left. The remaining solution produced, to the last drop,

crystals of alum \*. I dissolved these crystals in water, and precipitated the argil by carbonate of ammonia. After being carefully washed, dried, and heated red hot a quarter of an hour, it weighed 12 grains.

8. The brownish matter, No. 6. insoluble in caustic pot-ash, seemed to be oxyde of iron; and after having been heated red hot, weighed  $24\frac{1}{2}$  grains. I powdered this mass, and poured on it some acetous acid, in order to detect magnesia; but nothing was dissolved. It was next treated with nitric acid, which dissolved the iron, but left 4 grains of silex. The iron being precipitated, dried, and heated red hot, weighed 20 grains, and was magnetic. Suspecting that some argil might still be mixed with it, from having escaped the action of the caustic pot-ash, I dissolved 5 grains in muriatic acid, and precipitated the iron by Prussian alkali. Having separated the blue precipitate, I boiled the solution with carbonate of soda, and obtained 1 grain of argil. These 20 grains consisted, therefore, of 16 grains of iron, and 4 grains of argil.

The remaining part of the iron was melted for an hour, with ten times its weight of nitre, in order to detect manganese. The mixture, however, when cold, was not greenish; and water made a colourless solution of the saline matter, which did not become turbid, when exposed some days to the action of the air †.

9. The two portions of white earth above mentioned, which seemed to be carbonate of lime, weighed together 16 grains. To separate magnesia, if any were mixed with this earth, I put it into a little water, and added fulphuric acid till slightly in excess. Sulphate of lime was produced. Having poured some alcohol into this mixture, I filtered it, and washed the sulphate of lime with more alcohol diluted with water. The filtered liquor was then boiled with carbonate of soda, but no magnesia was precipitated.

These 16 grains were therefore carbonate of lime, of which, according to Mr. Klaproth's calculation, about 9 grains were pure lime.

One hundred parts of the basalt of Staffa contain, according to the above analysis :

Silex,	-	-	(No. 4. 7. and 8.).	-	-	48
Argil,	-	-	(No. 7. and 8.).	-	-	16
Oxyde of iron,	-	-	(No. 8.).	-	-	16
Lime,	-	-	(No. 9.).	-	-	9
Moisture, and other vol. matter,	-	-	-	-	-	5

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94

The sum is 94 parts; consequently there is a loss of 6 per cent.

\* It therefore contained none of the earth which Vauquelin lately discovered, and to which he has given the name of glucine.

† I think, however, it is probable, that this basalt contained a small quantity of manganese, both from the brownish colour of the solution, No. 1. and from the green colour which the undissolved residuum gave, by fusion, with caustic pot-ash.



About a year ago I analyzed specimens of some of the whins in the neighbourhood of Edinburgh, and found, that the sum of the earths and iron, separated by the analyses, never amounted to more than 93 or 94 *per cent.*; so that the loss was always equal to that just mentioned. It was this circumstance which first led me to suspect that some saline substance existed in these stones; and their considerable fusibility favoured the suspicion.

Soon after these analyses were made, I observed another circumstance, which amounted to an absolute proof of the whins containing something of a saline nature, in combination with their earthy bases. Most of the artificial crystallites, made by Sir James Hall, which I had always an opportunity of examining, threw out on their surfaces, two or three weeks after their formation, a white efflorescence, which had a very salt taste. It was in too small a quantity to be collected and examined; but when washed off, it was often formed a second time.

I was thus convinced of the existence of some saline substance in these bodies, and made different experiments with several of them, in order to separate it, and ascertain its nature; and soon found that it was soda.

I shall next describe some of the methods by which this alkali was most easily separated from the earthy parts of the whins.

*Experiments to obtain the Soda, and determine its Quantity.*

Having broken some of the basalt of Staffa to small fragments, I weighed 400 grains, and ground the whole with water to an extremely fine powder, in a Wedgwood mortar. The powder, and the water with which it had been ground, were then put into a small retort, and mixed with about 1200 grains of sulphuric acid, which I had carefully distilled for this operation. I placed the retort in the sand bath of a small furnace which I use for analyses, adapted a receiver, raised the fire till the acid began to distil slowly, and carried on the distillation to dryness. Water was then poured into the retort, and boiled, the mixture thrown on a filter, and the undissolved residuum sufficiently washed. This residuum was next treated a second time with a fresh portion of sulphuric acid; and afterwards boiled with water, filtered and washed, exactly in the same manner as before. The undissolved part of the stone was now almost white.

The filtered solutions being mixed together, were evaporated to dryness; and the saline mass which remained was heated red hot for one hour in a clean and new Hessian crucible. When cold the mass was of a brick red colour. Having powdered it well, I boiled it in water, poured the whole on a filter, and washed the reddish matter carefully. This filtered liquor, in which all the soda, separated from the basalt by the sulphuric acid, was dissolved in the state of sulphate of soda, could contain only a small quantity of earthy matter; for the greater part of the sulphate of argil, and of iron, formed by the first part of the process, must have been decomposed by the red heat, to which the mass was afterwards exposed. Accordingly, the solution being treated with carbonate of ammonia, only a small quantity of a precipitate was thrown down, which was carefully separated. The

solution was then evaporated to dryness. A saline mass remained, consisting, in part, of sulphate of ammonia; to separate which the whole was exposed to heat in a small crucible, and when it ceased to emit fumes, the heat was increased to redness. A fixed white salt was left, which weighed 25 grains.

This salt I re-dissolved in water, added some carbonate of ammonia, and heated the mixture till it boiled. A small quantity of an earthy precipitate was again thrown down, which being separated by filtration, the solution was evaporated to dryness, and the salt which remained heated red hot a second time. By these successive operations, all the earthy matter, at first dissolved, was separated. The salt now weighed 23 grains, and had all the properties of sulphate of soda. These properties were the following:

1. It was not volatile in a moderate red heat.
2. After being thus dried, it dissolved readily in about six times its weight of water, at the temperature of 60 of Fahrenheit.
3. This solution gave, by evaporation, crystals exactly the same in form as artificial sulphate of soda; and these crystals effloresced in dry air.
4. A part of the solution of this salt being boiled with carbonate of soda, gave no precipitate; a proof that it contained no earthy matter.
5. Some of the solution being mixed with a very strong solution of acid of tartar, remained unaffected; the salt therefore contained no pot-ash.
6. Some of the salt being dissolved in water, was decomposed by nitrate of barytes; and the sulphate of barytes produced was separated by filtration. The nitric acid, thus united to the alkaline basis, formed a saline compound, which, in the next place, was mixed and deflagrated with charcoal. By washing the coaly residuum, and evaporating the water, I obtained pure carbonate of soda, which effloresced readily in the air.

There can be no suspicion of the retort which was used furnishing any part of the alkali; for I weighed it previously in a balance of great accuracy; and after the operation was finished, found its weight exactly the same as at first, and the lustre of the glass altogether unimpaired.

The whin which I next submitted to examination, for the purpose of separating the soda, was taken from a quarry near the Water of Leith\*. I used a considerable quantity, 800 grains; which were distilled twice with sulphuric acid, and then treated in every respect exactly as the preceding. The sulphate of soda obtained, amounted to 43 grains.

I afterwards subjected some other whinstones to the same kind of processes, and in each species found soda. The nitric and muriatic acids also dissolve a certain quantity of the alkali contained in these substances; but their action is weaker than that of the sulphuric acid.

\* This species is the first mentioned in Sir James Hall's paper. When powdered, it effervesced slightly with acids. I did not analyze it; but, in the course of the process for detecting soda, one of the earthy precipitates proved, upon examination, to be magnesia. It is the only whin in which I have found this earth.



By the experiments now described, there were separated from 100 parts of each of the whins, between five and six parts of sulphate of soda, which may be equal to two or three parts of pure soda. But as these two or three parts, when added to the sum of the earths and iron, did not account for the loss of 6 or 7 *per cent.* always observed in my analyses, I was satisfied that the whole of the alkali was not obtained by the processes which were followed; even although, in that with the whin from the Water of Leith, it had been exposed, in very fine powder, to the action of the sulphuric acid, at a boiling heat, for more than eighteen hours. It appeared necessary, therefore, to try other methods; and after some consideration it occurred to me, that if the powdered whins could be exposed, while red hot, to the vapours of the sulphuric acid, also in a red hot state, its power in separating the whole of the alkali from the earthy bases of these substances, would probably be greatly increased in so high a temperature. I succeeded in applying a red heat both to the powdered stone, and to the acid at the same time, by the following means.

Some of the basalt of Staffa being mixed, in very fine powder, with three parts of sulphuric acid, the mixture was evaporated slowly to dryness in a sand bath. The dry mass was then heated gradually to redness, and kept in the fire for one hour. It was next powdered, and boiled in water; and the water being filtered, was treated with carbonate of ammonia, which threw down a small quantity of a brownish precipitate. After separating this precipitate by filtration, the liquor was evaporated to dryness, and the sulphate of soda, which was left, was purified in the manner already described, and heated red hot. It amounted to 9 parts for every 100 parts of the basalt employed.

In this experiment, the sulphuric acid was first united to a part of the lime, of the argil, and of the iron, contained in the stone; and afterwards, when the mass was exposed to a red heat, the acid was driven off partly or wholly from these, and applied in red hot vapours to every part of the powder; by which its action appears to have been rendered much more powerful, as 9 *per cent.* of sulphate of soda was produced: and by the same process, so simple and easy to execute, I got from the rest of the substances, to be mentioned in this paper, from 8 to 11 *per cent.* of sulphate of soda, although, when they were merely boiled in the acid, the quantity of this salt never exceeded 5 or 6 *per cent.*

As the proportion of acid and alkali in neutral salts has not been hitherto determined with certainty, the quantity of soda in these whins cannot be exactly known. But it is probable that 9 parts of sulphate of soda, dried by a red heat, do not contain less than  $3\frac{1}{2}$  or 4 parts of pure alkali\*; which must therefore be considered as the weight in 100 parts of the basalt of Staffa; and as  $3\frac{1}{2}$  or 4 parts of soda, when added to the sum of the earths and iron, amount nearly to the 100 parts of the stone employed in the analysis, this calculation may be reckoned very near the truth. For the same reason I think it likely, that the greater part, or the whole of the soda, was obtained from the basalt by the process which has been last described.

\* This is nearly the proportion given by Mr. Kirwan.

It is well known among the friends of the late Dr. Hutton, that he made some experiments on zeolite; by which he concluded, that soda entered into the composition of that substance\*. He has not mentioned the circumstance in any of his works; but Dr. Black has been accustomed, as he informed me himself, to take notice of it in his lectures on chemistry, for many years. It is my intention to analyze some species of zeolite; and if the results seem of any importance, they shall be laid before the Society.

Among the experiments on the basalt of Staffa, already described, it has been observed, that, when the powder was boiled in water, a slight precipitate was produced in the water by nitrate of silver, thus indicating some traces of muriatic acid. As it appeared of importance to determine how much of this acid the basalt contained, I subjected some of it to examination for that purpose.

*Experiments to ascertain the Quantity of Muriatic Acid in the Basalt of Staffa.*

One hundred grains of the stone, in fine powder, were mixed in a small retort with some nitric acid; and a receiver being adapted, the mixture was boiled gently, till the greater part of the acid had distilled over. The liquor in the receiver being examined with nitrate of barytes, remained unaffected; but gave a slight cloud with nitrate of silver, which shewed that it contained some muriatic acid.

The mass in the retort being diluted with water, the whole was filtered; and this filtered liquor produced no cloud with nitrate of barytes, but gave, like the former, a slight precipitate with nitrate of silver.

In the next place, the undissolved residuum was mixed with twice its weight of very pure caustic pot-ash, and exposed to a low red heat, for an hour, in a silver crucible. The mass was then diluted with water, superaturated slightly with nitric acid, and filtered. With this solution nitrate of barytes produced no effect; consequently, these experiments show, that the stone in question does not contain any traces of sulphuric acid. With nitrate of silver, however, the solution gave a white precipitate, more abundant than the two preceding. The different portions of muriate of silver, being collected, and dried on a sand bath, weighed only 4 grains.

As a fourth part of muriate of silver consists of acid, according to the most correct experiments hitherto published, these 4 grains consequently indicate, in 100 parts of this basalt, only about one of muriatic acid. All the whins and lavas, to be mentioned in the remaining part of this paper, were found, by a similar process, to contain about the same quantity.

\* In the 26th volume of the *Annales de Chimie*, p. 119. M. Scherer, in a letter to Van Mons, says, that he was informed by Dr. Black, that Dr. Hutton had, long ago, found *pot-ash* in zeolite. In this statement M. Scherer is incorrect; because it was soda, as above-mentioned, which Dr. Hutton obtained from that substance.



According to the results of these different processes, there are, in 100 parts of the basalt of Staffa,

Silex,	-	-	-	-	-	-	48
Argil,	-	-	-	-	-	-	16
Oxyde of iron,	-	-	-	-	-	-	16
Lime,	-	-	-	-	-	-	9
Moisture, and other vol. matter,	-	-	-	-	-	-	5
Soda, about	-	-	-	-	-	-	4
Muriatic acid, about	-	-	-	-	-	-	1
							99

I have thus detailed exactly the various experiments performed in analyzing this species. As all the other whins, and the lavas, which follow, were analyzed in the same manner, and exhibited nearly the same chemical properties, the results only, in each example, shall be mentioned.

## ANALYSIS II.

### *Whin of Salisbury Rock.*

The specimen employed was chosen from the south side of the cragg, and was perfectly hard and free from decomposition, as the particular spot from which I broke it had been quarried a short time before.

A description of its external characters may be found in p. 54 of the 5th vol. of the Transactions of the Royal Society of Edinburgh, (or p. 15 of our present vol.) Its powder is light greenish grey; but after being wet, acquires a dirty green colour. When heated to redness, it becomes light brown. Though not attracted by the magnet in its natural state, it becomes magnetic after being heated red hot. It does not effervesce with acids. When exposed to a low red heat for half an hour, whether in fragments or in powder, it loses 4 per cent. in weight. It softens at 55 of Wedgwood. Its specific gravity is 2.802. After being boiled, in the state of fine powder, in ten or twelve parts of muriatic acid, the insoluble residuum amounted to 65 per cent.

I analyzed 200 grains of this whin, and found that it contained, in 100 parts,

Silex,	-	-	-	-	-	-	46
Argil,	-	-	-	-	-	-	19
Oxyde of iron,	-	-	-	-	-	-	17
Lime,	-	-	-	-	-	-	8
Moisture, and other vol. matter,	-	-	-	-	-	-	4
Soda, about	-	-	-	-	-	-	3.5
Muriatic acid, about	-	-	-	-	-	-	1
							98.5

(To be concluded in our next.)

Further

## VII.

*Further Remarks on the Enquiries of Dr. Herschel respecting Light and Heat. In a Letter from Mr. JOHN LESLIE.*

TO Mr. NICHOLSON.

S I R,

London, Nov. 17, 1800.

MY last Letter was written under the persuasion that your Journal for October contained, at least in substance, the *whole* of Dr. Herschel's late experiments and conjectures. Had I foreseen that the subject would be resumed in a future number, I should certainly have deferred my remarks until the recital was closed. I now, therefore, feel myself reluctantly compelled to reconsider the question; and to state still farther difficulties, shall I say fallacies, which press the sequel. In most cases, the toil of criticism is miserable waste of time; in every case, it is equally painful and inglorious; and in coming forward to attack the solidity of facts and conclusions sanctioned by high authority, I shall probably, with men of a certain class, incur the charge of temerity and presumption. But such prudential considerations I utterly disregard; being convinced that, on occasions like the present, I may promote the cause of genuine science as effectually by detecting errors as by announcing positive discoveries. Fortunately, I need not at this time engage in much elaborate discussion. The objections which I formerly urged subsist in their full force, nor do I find on recollection any material assertion which I should desire to correct.

The additional experiments which claim examination contain little indeed that can be strictly called original. They are employed for the most in ascertaining facts which have been long established; or which are familiar to every person who has the smallest tincture of science. The paper opens with a formality and apparent caution that might lead us to expect a chain of proofs scarcely inferior to mathematical evidence. We are soon forcibly reminded however, that *such* is not always the surest road to truth; and that the minute spirit of subdivision has prevailed most in the dark ages, and in the barbarous departments of literature, when sense and reason were alike buried in the *verbiage* of scholastic syllogisms, definitions, and distinctions. To the mystic number *seven*, the child of judicial astrology, the Doctor bows with reverence. Light not only consists in seven primitive rays, but each ray has seven *properties*; and so likewise corresponding have the "rays of heat." To muster up precisely those seven analogous properties, however, required some degree of management, since one of them is to inform us, that the rays of light and those of heat "are liable to be scattered on rough surfaces;" an expression which, if it has any meaning at all, must denote irregular reflection, and therefore, to common apprehension at least, seems comprized under a former head. The parallel so nicely drawn between the visible and invisible rays, changes, in the last article, into a curious contrast, which asserts, in despite of vulgar prejudices, that Light *may not* give Heat, and yet that Heat *may* compose Light.

OF



Of twenty experiments which are related in detail not fewer than sixteen have no direct connection with the question agitated. They refer merely to the heat occasioned by the rays of light from the sun, a candle, a fire, or a red-hot poker, when condensed in the focus of a lens or speculum; or to the heat collected by convergent reflexion from a heated mass or from the vicinity of a luminated substance. In all this, I can discern nothing either new or striking. But the Doctor thinks fit or convenient to substitute the word heat for light; and that easy change operates like a magical charm. Whatever property has been attributed to the rays of light, belongs henceforth exclusively to the "invisible rays of heat," and thus comes directly in support of his darling hypothesis. Such a mode of reasoning, or rather assumption, hardly deserves any serious refutation. But the Doctor seems quite transported with the discovery. In the eleventh experiment, entitled, "On the Refraction of Solar Heat," a large Newtonian telescope, with a compound eye-piece, was directed to the sun, and by the concentration of the broad beam of light in the focus, a very considerable heat, as every body knows, was produced. The philosopher stops, as usual, to wonder. "How artfully," he exclaims, "in our present instance, was heat sent from one place to another!" And, borrowing the language of Newmarket, he continues:—"Heat crossing heat, through many intersecting courses without jostling together, and each parcel arriving at last safely to its destined place." The grand conclusion is, that "it cannot be doubted that the rays of heat are subject to the laws of refraction." In the whole of this strange passage, perhaps the only thing which should excite surprise in a sober mind, is the glaring confusion of ideas.

The 7th and 8th experiments, which undertake to "reflect and condense the invisible solar rays," have nothing remarkable but their title. I formerly showed, I hope in a convincing manner, that those imaginary invisible solar rays were merely the warm portions of air which environ an illuminated body. Of course, this heat may, as in other cases, be collected by reflection. It is not my present design to discuss the nature and propagation of heat. But I cannot forbear mentioning a single argument, which, if I mistake not, is decisive against the adherents of Radiant Heat. No fact is better known than that the rays of light, in traversing an uniform medium, are not in the smallest degree affected by its most violent agitation, but pursue unvaried their rectilineal course. The case is very different with what is termed radiant heat. The experiments on which it rests succeed only in a close room, and at very moderate distances from the source of heat. It is clear, therefore, that the heating matter must flow with such feeble impulse as to experience obstruction in its passage through the air, and suffer much derangement from the accidental motions of that fluid. If this supposed radiant heat darted with a celerity in any degree comparable to that of light, nay even to the velocity of those projectiles with which we are acquainted,—it would be exactly directed and concentrated in open air, and that at distances limited only by the unavoidable imperfection in the figure of the reflecting surface. Hence, the heating matter, whatever it really is, must have a grossness of constitution, and a slowness of progress, commensurate with the denseness and ordinary mobility of our atmosphere.

The 17th, 18th, 19th, and 20th, are the only experiments which require any particular notice, as seeming to countenance Dr. Herschel's paradoxical opinions. In the first, a semicircular piece of pasteboard, covering the half of a large lens, received the coloured prismatic speculum, but permitted the "invisible rays" beyond the margin of the red to pass through the glass. A heat of 45 degrees was caused in the focus. But with what care and nicety the experiment was performed may be judged from the circumstance, that the bulb of the thermometer appeared illumed by a reddish tint. This perplexing occurrence, however, only inflames the love of the marvellous. The Doctor very gravely proceeds to enquire whether *invisible* rays can, by condensation or accumulation, become *visible*. He must entertain indeed a lofty idea of the nature and value of experiment, thus to set it in opposition to what are accounted the dictates of common sense. Had the result proved successful, what a triumph gained over the frailty of human reason! It would have taught us to listen with humility to the dreams of a Platonist or visionary of the present day, who announces the new and sublime discovery, that the addition of *nothings* makes *something*. But Dr. Herschel's surmise was not confirmed; and on repeating his experiments with some little more attention, the effect was only 21 degrees, being not the half of what was before produced. This, among other instances, may serve as a sample of the author's accuracy and circumspection. But how did he discover that pasteboard would intercept the *whole* of the incident light? He confidently regards this cover, applied too on the very surface of the burning glass, as a perfect diaphragm. Yet when an experiment, and the only one in any degree conclusive, is adduced, tending to support an opinion most repugnant to our general ideas, we might reasonably expect that every precaution would be used, and the previous steps at least scrupulously determined. Common writing-paper, I have found, transmits about one-half of the whole incident beam. What was the thickness or the texture of the Doctor's pasteboard, we are not informed. But even granting his experiments to be performed with accuracy, if the pasteboard permitted only the sixth part of the light to pass, this would have been sufficient to produce the alledged effect. And after all, what reliance ought to be placed on observations which are at variance with every known fact, and every established principle?

The 19th experiment, whose object is "the refraction of invisible culinary heat," absolutely proves nothing. A hot cylinder of iron was placed near 3 inches from a lens above an inch in diameter, and a thermometer at an equal distance behind it in a position corresponding to the secondary focus. The thermometer rose a degree or two as the iron cooled, and diffused its heat among the neighbouring bodies. Nor did it require any vast stretch of ingenuity, still less an actual appeal to experiment, to perceive that, each time a small screen was interposed, the bulb would suffer a certain depression of temperature.

The 20th, and last experiment is intended to confirm the preceding; with what success will be presently seen. Another thermometer was placed near the former, but constantly exposed to the stream of heat: In 4 or 5 minutes it acquired its maximum rise, amounting to about a degree and a half. The other thermometer which was screened and exposed alter-



nately, gained an equal increase of heat; but, for an obvious reason, required to that effect double the time. The observation was next repeated by placing both thermometers in a situation to be equally affected by the screen, the one in the focus of the lens and the other beside it. The thermometers kept pace together in their progress, and their fluctuations, as often as the screen was interposed or withdrawn, corresponded with tolerable precision. The irregular difference of perhaps a quarter of a degree is surely a quantity too small to form the ground of any legitimate inference. Yet mark with what confidence Dr. Herschel pronounces his precipitate conclusion: "All which so clearly confirm the effect of the refraction of the lens, that it must now be evident that there are rays issuing from hot iron, which, though in a state of total invisibility, have a power of occasioning heat, and obey certain laws of refraction, very nearly the same with those that affect light." It is truly astonishing to observe how the mind, when once occupied by some favourite idea, recalls it at every step, and tortures every slight appearance into an argument for its support.

In another paper since read at the Royal Society, Dr. Herschel pursues his subject with the same monotonous prolixity. It chiefly consists of experiments directed to ascertain the quantities of light transmitted through different coloured glasses. But such researches are of no real utility in a philosophical view, unless many other points had been fixed which the Doctor entirely overlooks. The thickness of the glass, its composition, and the intensity of its shade, were of the most essential consequence. To determine that depth of transmitted colour, would have required a comparison with the triangular cells of Mayer, as improved by Lambert, Achard, and Burja. The variable force of the sun's rays ought also to enter into the account, whether as affected by his different altitude above the horizon, or by the condition of our atmosphere, and principally in respect to humidity. I have observed a very sensible difference in the power of solar light in consecutive days at noon, even when the sky was apparently clear. If colours offend the eye by excess or defect, some gross estimate may be made of their respective degrees of illumination; but to determine those in general and with precision by ocular contrast, seems altogether an impracticable, if not an absurd, attempt. In fact, what just comparison can obtain, for example, between green and red, which are strictly things as heterogeneous as taste and smell. To increase the embarrassment, we cannot always judge of the colour of the transmitted light by that of the substance through which the rays are sent. Thus a sun-beam, in emerging from pasteboard, ivory, or white enamel, will, according to their thickness, exhibit all the tints from yellow to deep red. Yet admitting, in its fullest extent, the accuracy of Dr. Herschel's results, they furnish no evidence whatever in support of his hypothesis, or indeed of any other hypothesis.

There is only a single passage in the concluding paper that requires particular notice. Dr. Herschel had anticipated one consequence which I urged as an insurmountable objection to his system; namely, that a burning glass would act most fiercely at some distance behind the bright focus. The Doctor tried this, and, as he asserts, found it to succeed; though he acknowledges at the same time, and with great truth, that the experiment was

but a *coarse* one. After such a confession, your readers will judge what degree of credit it really deserves. Is it at all likely, may possible, that so many able philosophers, who for above a century back, have employed their talents on the improvement and application of the burning glass, should have totally overlooked a fact so obvious and so palpable. The Doctor has a remarkable faculty, not peculiar to him however, of flurring over those points which immediately affect his opinions, and of dwelling with minuteness on what all the world may be presumed to know. Had he uniformly exercised the same laudable scepticism which he professes at the outset, he might perchance have doubted whether his precursors were not sometimes right, and himself mistaken. It would require more than ordinary docility to believe Dr. Herschel's "coarse" experiment, in opposition to all the former concurrent testimonies.

I now take leave, I hope for ever, of this controversy. Without questioning Dr. Herschel's fidelity, I have shown that his assertions are not only inconsistent with all our general and best founded notions, but stand directly contradicted by actual observations made with peculiar advantages; that his experiments were injudiciously contrived, executed without circumspection, and liable to a multiplicity of inaccuracies; that his reasonings, how boldly soever advanced, were still more defective; that his later experiments are the more vulnerable in proportion to the confident tone which he assumes; that by far the major part of them is totally unconnected with the subject in dispute, and only twisted to serve his purpose by the sophistical transposition of terms; and that the few which actually apply are of such obscure and ambiguous character as to afford no certain evidence. In short, those experiments and conjectures, taking their combined impression, may for a while gratify vulgar curiosity, but must soon hasten to final oblivion. I should be sorry if my strictures gave offence to Dr. Herschel. If I have spoken with freedom, I trust it is in the language which conviction and the love of truth naturally inspire. Undue authority, always depressing, is capable of producing most fatal effects, when suffered to gain possession of the sciences. How long did the ascendancy of Aristotle, of Des Cartes, and shall I add in some few points that of the venerable name of Newton, retard the advancement of real knowledge. I respect Dr. Herschel's talents, I admire his astronomical discoveries, and I am persuaded that England, in the decline of her science and philosophy, needs the importation of genius from abroad, and is honoured by becoming his adopted country. If I cannot equally approve of several of his late speculations, I reflect that men seldom estimate aright their own powers, seldom know where their real strength lies. Adventuring on new subjects they are not likely all at once to acquire the skill, precision, and caution which are generally the fruit of experience and patient application.

But I cannot finish the letter without addressing a few words to you, Sir, as editor of a respectable Journal. I advert to the note with which you have honoured me at page 348. I certainly presumed that you gave unchanged the ideas, if not the words of your author. In this it appears I was mistaken; and I cheerfully retract the expression to which it gave rise, and which were merely the spontaneous effusion of the moment. Dr. Herschel only  
*supposes,*



*supposes* "that the cooling causes *must* must have a stronger effect on the mercury in a small bulb;" but *why* they should have such effect, is left for others to discover. Nor do I think, Sir, that you were happy in the attempt to extricate him from the dilemma. I readily grant that currents of air may ascend, which, though warmer than the encircling mass of atmosphere, are colder than the body itself. But I maintain that those currents *have already* received their heat, and ascend only *in consequence* of that communication.—The air which touches the hot body receives its full share of heat, and, at the same instant, its force of ascension: after it has begun to mount, the effect has ceased; nor can it exert its cooling energy unless it again descends, to renew the process\*. To argue otherwise would require the assistance of a sort of aitiology like that with which some chemical writers have amused us of late years concerning pre-disposing or pre-existent affinities. But I will not dwell on a dispute of trivial moment. The author of a periodical work, who is obliged, frequently perhaps without premeditation, to satisfy the urgent calls of his compositor, is entitled to much indulgence. I most readily excuse the passage which was criticised, and which might inadvertently slip from your pen; and by printing the above, you will give to the public one proof more of your impartiality and candour.

I am, SIR,

Your most obedient Servant,

JOHN LESLIE.

[Erratum—in last Number, page 346, line 9, before *visual* read *same*.]

## VIII.

*Experiments and Observations on the Light which is spontaneously emitted, with some Degree of Permanency, from various Bodies. By NATHANIEL HULME, M.D. F. R. S. and A. S.†*

### INTRODUCTION.

THE discoveries which have been made with respect to light, as it proceeds immediately from the sun, are many and important; but the observations on that species of light which is spontaneously emitted from various bodies, are not only few in number, but in general very imperfect. The author is therefore desirous of drawing the future attention

\* The air, which is first heated, ascends upon the same principle as other floating bodies; namely, because its specific gravity is diminished, and the upward pressure of the fluid beneath, is, therefore, greater than that of the super-incumbent column added to that of the heated mass. The lower part of the whole ascending current (which Mr. L. seems to overlook) is as cold as the rest of the atmosphere; and it is obviously this portion which maintains the cooling process, by striking the inferior surface of the thermometer in its ascent.—N.

† Philof. Transf. 1799, p. 161.

of the philosopher more particularly to this subject, and of communicating his own experiments and observations upon it, to this learned Society.

By the spontaneous emission of this light, the author wishes to distinguish it from all kinds of artificial phosphorus; which, as he apprehends, differ essentially, in some of their properties, from that light of which he means to treat. And, by its adhesion to bodies with some degree of permanency, he distinguishes it from that transient sort of light which is observable in electricity, in meteors, and in other lucid emanations. The light which is the subject of this paper, he shall therefore beg leave to discriminate by the name of *spontaneous light*.

The substances from which such light is emitted, are principally the following.

Marine animals, both in a living state, and when deprived of life. As instances of the first may be mentioned, the shell-fish called *pholas*, the *medusa phosphorea*, and various other *mollusca*.

When deprived of life, marine fishes in general seem to abound with this kind of light. The honourable Mr. Boyle commonly obtained light, for his use, from the whiting, as appears from many parts of his works: the author of these experiments and observations procured his fish light chiefly from the herring and the mackerel.

The flesh of quadrupeds has also been observed to emit light. Instances of this are mentioned by Fabricius *ab Aquapendente*; by T. Bartholin; by Mr. Boyle; and by Dr. Beale; for which, see T. Bartholin, *de Luce Animalium*, p. 183; Boyle's Works, Vol. III. p. 304; Phil. Transf. Vol. XI. p. 599.

In the class of insects are many which emit light very copiously, particularly several species of *fulgora* or lantern-fly, and of *lampyrus* or glow-worm; also the *scolopendra electrica*; and a species of crab, called *cancer fulgens*.

Rotten wood is well known to emit light spontaneously. Peat earth also has the same property. Of the effects of the latter, a remarkable instance is related in Plot's Natural History of Staffordshire, p. 115.

The place where the following experiments were made, was a dark wine-vault, which, for distinction's sake, the author calls the *laboratory*. The heat of this laboratory varied, throughout the year, from about 40 degrees of temperature to 64°. The thermometer made use of was that of Fahrenheit.

The weight is always to be supposed that called troy weight. The liquid measure employed, was that used for wine in this country: the ounce containing 8 drams avoirdupois; and the pint, 16 ounces.

The water used in general for the experiments, was pure spring water, drawn up from under ground by means of a pump; and it was always employed cold, unless otherwise expressed.



## SECTION I.

*The Quantity of Light emitted by putrescent Animal Substances, is not in Proportion to the Degree of Putrefaction in such Substances; as is commonly supposed; but, on the contrary, the greater the Putrescence, the less is the Quantity of Light emitted.*

### EXPERIMENTS.

*Exper. 1.* Two very fresh herrings were bought in the morning, and hung up in the laboratory; on examining them in the evening, they were beginning to be luminous.

*Exper. 2.* Three herrings, which were quite fresh, after being scaled and gutted, were hung up by a string in the laboratory. The next evening they were become exceedingly luminous in every part, and much lucid matter had exuded, as it were, upon their surface, which was easily scraped off by the blunt edge of a knife; it also adhered to the fingers, or other parts of the body, when touched; but, as they grew more putrescent, the quantity of light diminished, and at last was extinguished.

*Exper. 3.* A single herring, that was perfectly sweet, was hung up in the laboratory. On the second night, it was covered with light; on the third, not so lucid; on the fourth, less so; and so on, in proportion to the degree of putrescence.

*Exper. 4.* Two herrings, somewhat stale, were hung up in the morning, and at 8 P. M. one of them was pretty luminous, but the other less so. On the next evening, the former was but slightly luminous, and the latter was dark; on the succeeding evening, they were both dark.

*Exper. 5.* Two mackerels were brought from the market at 1 P. M. which, to the sight and smell, were perfectly sweet and good. Being then carried into the dark laboratory, and examined, the one was found to be a little luminous, and the other pretty much so especially about its belly.

*Exper. 6.* A fine fresh mackerel, with a bright eye, was purchased about noon, and placed as usual in the laboratory, the temperature of which, at that time, was about 54°. At 11 P. M. this beautiful fish was luminous about the head and upper parts; and the inside of the mouth, which was wide open, shone with most brilliant light. The next evening, the whole body of the fish was very luminous: on the third night, it was less so; and on the fourth the light was nearly extinguished.

*Exper. 7.* In the forenoon, about ten o'clock, a couple of fine-looking mackerels were hung up in the laboratory, at the temperature of 56°, and at 10 P. M. they began to shine in various parts, the light seeming to proceed from within outwards. On the second night, they put on a luminous appearance all over their surface: on the third, the light was not so vivid; and on the fifth it was almost extinct.

N. B. In experiments of this kind, for the production of light, the fishes should always be gutted, the roes taken out, and the scales, if any, carefully removed. As the roes are likewise very productive of light, they should be preserved.

OBSER-

## OBSERVATIONS.

*Obs. 1.* These experiments clearly prove, that light begins to be emitted by marine fishes, before any signs of putrefaction appear: they likewise demonstrate, that as soon as a great degree of putrescence has taken place, the luminous property of the fishes is destroyed, and the light extinguished.

*Obs. 2.* In the instance of light proceeding spontaneously from animal flesh, recorded by Aquapendente, the flesh emitted light before any sensible putrescence had taken place, the meat being hung up in the larder for use. In that also mentioned by Bartholin, in 1641, the flesh must have been fresh and sweet, for it was not intended to be dressed until the next day. Mr. Boyle, in his report of light issuing from flesh, expressly says, that neither he, nor any of those who were about him, could perceive in it any offensive smell, whence to infer any putrefaction; the meat being judged very fresh, and well conditioned, and fit to be dressed. And, lastly, Dr. Beale, in his account of a luminous neck of veal, says, that when it was dressed, on February the 27th, some of the neighbours, who saw it shining, were invited to eat of it, and all esteemed it as good as they had ever tasted; that a part of it was kept for February 28th and 29th, in which time it lost nothing of its sweetness.

*Obs. 3.* Whenever I wish to obtain a plentiful supply of light from fishes, for the purpose of experiments, I always endeavour to procure the freshest that can be had: long experience and frequent disappointments have taught me to adopt such a precaution.

## SECTION II.

*The Light here treated of is a constituent Principle of some Bodies, particularly of Marine Fishes, and may be separated from them, by a peculiar Process; may be retained, and rendered permanent for some Time. It seems to be incorporated with their whole Substance, and to make a Part thereof, in the same Manner as any other constituent Principles.*

## EXPERIMENTS.

*The Flesh of Herring\*.*

*Exper. 1.* A fresh herring was split, or divided longitudinally, by a knife, into two parts. Then, about four drams of it, being cut across, were put into a solution, composed of two drams of Epsom salt or vitriolated magnesia, and two ounces of cold spring water drawn up by the pump. The liquid was contained in a wide-mouthed three-ounce phial, which was placed in the laboratory. Upon carefully examining the liquid, on the second evening after the process was begun, I could plainly perceive a lucid ring (for the phial was round) floating at the top of the liquid, the part below it being dark; but, on shaking the phial, the whole at once became beautifully luminous, and continued in that

\* The quantity used in each experiment was about four drams.



state. On the third evening, the light had again risen to the top; but the lucid ring appeared less vivid, and, on shaking the phial as before, the liquid was not so luminous as on the preceding night.

*Exper. 2.* The same experiment was repeated. On the second night, the liquid, being agitated, was very luminous; on the third, not so lucid; and on the fourth, the light was extinguished.

*Exper. 3.* With sea salt or muriated natron half a dram, and two ounces of water. On the second night, the liquid, when agitated, was dark; on the third, lucid; on the fourth, very luminous; on the fifth, it began to lose light; on the sixth, it continued to decrease; and on the seventh it was quite gone. Neither the liquid, nor the herring, had contracted any putrid smell.

*Exper. 4.* With sea water two ounces. On the second night, dark; on the third, fourth, and fifth, luminous; on the sixth, nearly extinct; and on the seventh, totally. The piece of herring, when taken out and examined, was remarkably sweet.

*Roe of Herring\*.*

*Exper. 5.* With Epsom salt two drams, and water two ounces. On the second night, the liquid was pretty luminous; on the third and fourth, still luminous; and on the fifth its light was extinct.

*Exper. 6.* With Glauber's salt or vitriolated natron two drams, to two ounces of water. On the second night, when the phial was shaken, as usual in all these experiments, the liquid was pretty luminous; on the third, less so; and on the fourth the light was scarcely visible.

*Exper. 7.* With sea water two ounces. On the second night, dark; on the third, the liquid was moderately luminous; on the fourth and fifth, it had extracted much light; and on the seventh it was still shining. After this process, both the roe and the sea water remained perfectly sweet.

*The Flesh of Mackerel.*

*Exper. 8.* With Epsom salt two drams, and water two ounces. On the second night, the liquid was finely illuminated; on the third, a similar appearance; on the fourth, a diminution of light; on the fifth, it continued lucid in a small degree; and on the sixth the light was extinguished.

*Roe of Mackerel.*

*Exper. 9.* With Epsom salt two drams, and water two ounces. On the second night, the liquid, when agitated, was exceedingly bright; on the third, the same; and on the fourth and fifth, still lucid.

\* The quantity used in each experiment was about four drams.

*The Tadpole.*

*Exper. 10.* It occurred to my mind, in the year 1797, to try what effect a saline menstruum would have upon the tadpole. Accordingly, I procured some tadpoles on the 10th of June, and put six of them into a solution of two drams of Glauber's salt in two ounces of water. On the 11th, in the evening, the menstruum was dark; on the 12th, after shaking the phial, I was agreeably surprised to find it impregnated with light; on the 13th, the light was so abundant as to float on the top of the menstruum; on the 14th, the same phenomenon appeared; on the 15th and 16th, it was still present; on the 17th, the lucidness began to diminish; on the 18th, it was faint; and on the 19th it had vanished.

*Exper. 11.* On the 11th of June, six other tadpoles were dropped into a solution of one dram of common salt in three ounces of water. On the 12th and 13th, the menstruum was dark; on the 14th, it had extracted from the tadpoles a very beautiful bright light; on the 15th, the menstruum was exceedingly luminous; on the 16th and 17th, nearly the same: the light then gradually faded, so that on the 21st it was merely visible; and on the 22d it disappeared.

*Exper. 12.* On the 21st of June, the above two experiments were repeated; when the tadpoles remained in the menstrooms till the 27th, but no light was emitted. What was the cause of this failure in these two last experiments? Was it the ten days' increased growth of the animal, which was taken from the same pond, that made the difference?

*Exper. 13.* The above experiments were repeated, when the tadpole had just put on the state of a frog, but without producing any lucid appearance.

*The Light is incorporated with the whole Substance of Marine Fishes.*

*Exper. 14.* A fine fresh herring, being gutted, was divided longitudinally into two parts, both of which were hung up, by pieces of string, in the laboratory. On the 2d night, they were very lucid on the skinny side, but not on the fleshy or inward part; on the 3d, the fleshy or central parts of the fish, were thickly covered with a rich azure light; on the 4th, they continued exceedingly luminous; and on the 5th and 6th they were still lucid. It is surprising to think what a profusion of light was emitted from the interior substance of this single fish.

*Exper. 15.* A similar experiment was made with a mackerel, and with similar effects. These two experiments were frequently repeated.

*Exper. 16.* But the soft-roë, of both the herring and the mackerel, abounds more with light than even the flesh. When it is in its most luminous state, which generally happens about the 3d or 4th night, it will sometimes shine so very splendidly, as to appear like a complete body of light. It is remarkable that the hard-roë, in general, does not emit so much light as the soft-roë. When the roës were used, they were laid upon plates, and deposited in the laboratory.



## OBSERVATIONS.

*Obs. 1.* The above experiments clearly prove, as I apprehend, that this light is a constituent principle of marine fishes: and that it is separated, by the menstruum employed on this occasion, in the same way that the principles of any other body are separated, by the menstruum fitted to decompose it. They likewise show, that it is not partially but wholly incorporated with every part of their substance, and makes a part thereof, in the same manner as any other constituent principle.

*Obs. 2.* Light is probably the first constituent principle that escapes, after the death of marine fishes. The experiments of the first Section teach us that it appears soon after death, even in fishes which, to the eye, seem quite fresh and sweet; or, at least, long before any sensible putrescence takes place. And we have seen that the flesh and roes, infused in the saline menstrua, continued to emit light for several days, without undergoing any apparent putrefactive change.

*Obs. 3.* The experiments likewise render it probable, that no offensive putrefaction takes place in the sea, after the death of such myriads of animals as must needs daily perish in the vast ocean, (quite contrary to what happens on land;) and that the flesh of marine fishes remains pretty sweet for some time, and may become wholesome food for many kinds of those which still remain alive. An eminent instance this, of the wisdom of the Creator, in the construction of the aqueous part of the world, which comprehends, by far, the greatest portion of the terraqueous globe, and is the most replete with animal life!

## SECTION III.

*Some Bodies or Substances have a Power of extinguishing spontaneous Light when it is applied to them.*

## EXPERIMENTS.

The luminous matter proceeding from the herring and the mackerel, was quickly extinguished when mixed with the following substances: 1. Water alone. 2. Water impregnated with quick-lime. 3. Water impregnated with carbonic acid gas. 4. Water impregnated with hepatic gas. 5. Fermented liquors. 6. Ardent spirits. 7. Mineral acids, both in a concentrated and diluted state. 8. Vegetable acids. 9. Fixed and volatile alkalis, when dissolved in water. 10. Neutral salts: viz. *saturated* solutions of Epsom salt, of common salt, and of sal ammoniac. 11. Infusions or chamomile flowers, of long pepper, and of camphor, made with boiling-hot water, but not used till quite cool. 12. Pure honey, if used alone.

*(To be concluded hereafter.)*

## IX.

*On Areometry; more particularly as it relates to Alcohol of different Strengths and Temperatures. By Cit. HASSENFRATZ \*.*

THE Author calls the instruments made use of for determining the proportion of mixtures of alcohol and water, alcogrades; and after a short preface respecting the uses of spirituous and vinous fluids, he proceeds to enumerate the following six methods of measuring the strength of brandies, &c. 1. By the bubble or bead which appears when the fluid is shaken. 2. The swimming or sinking of oil poured therein. 3. Distillation, to show the quantity of spirit. 4. Burning the fluid in a silver vessel, and noting the residue. 5. Wetting a known quantity of dry gunpowder with a little of the spirit, and observing the facility or difficulty with which it inflames. These five methods are, as he remarks, not only too uncertain to shew the difference of spirit with the desired precision, but will even give very different results, according to the management, with samples of the very same spirit. 6. The sixth method consists in determining the specific gravity with the Areometer, which as it has an appearance of precision, deserves to be more minutely examined.

The Areometer or instruments for determining this specific gravity of fluids are of two kinds, namely, 1. a floating ball, with a stem above, either graduated or supporting a dish to receive weights, and a counterpoise below to preserve the erect position; and 2. a bottle terminating in two small apertures, and capable of holding the same invariable bulk of fluid.

The first, namely, the hydrometer with the graduated stem is most in use. Instruments of this kind have been graduated in France by observing certain points to which the subsidence was made in water and in alcohol, or in a saline solution. Nine of those more generally used in that country are exhibited in the following table:

\* Abstracted from the fourth Memoir on this subject, in the *Annales de Chimie*, XXXIII. 31.



Table of Comparison for the Areometers of

Names of the Liquors.	Names of the Inventors or Companies.										Specific gravity
	Lantenay	Cartier	Baumé	Buffat	Machi		Mac Daniel	Juges d'Aunis	Marchands de Paris	Struve	
Rectified Alcohol	80	36	40	100	66	0	33	16	130	37.5	0.8276
Melasses Spirit	78	35	38	93	64	1	30.75	15.5	127	35.5	0.8372
Common Alcohol	74	33	35.3	87	62	2	27.8	13	121	33	0.8508
Cognac { 6—11	65	31	32.75	79	52	7	25	12	106	30.75	0.8636
Brandy { 4—7	60	30	32	75	49	9	24	11.75	100	30	0.8675
Brandy of Barcelona	61	31	32.75	79	51	7.75	25	11.5	102	30.7	0.8636
— of Montpellier	59	29.75	31	75	47	9.75	23	11.3	95	29	0.8727
— potable simp. 4 years	30	20	20.5	40	23.75	22	11.3	3.5	48	19.6	0.9320
— of 20 years	28	20	20	40	23	22.75	10.5	3.5	46	19.1	0.9351
— artificielle 6—11	25	19	18.75	34	20	23.75	9.3	2.5	40	17.	0.9427
Red Champagne wine	5	12	11	10	2	33	0.5	0	13	11.5	0.9931
White Burgundy	4	11.75	11	9	2	33	0.5	0	14	11.	0.9931
White Orleans Vinegar	0	10	9	2	0		0	0	0	9.	1.0070
Distilled Water	0	10	10	5	0	34	0.5	0	0	10.	1.0000

From the imperfection of these instruments, particularly with regard to the indication of the component parts of spirituous mixtures, it has been an object of importance to philosophers, and the projectors of legislative imposts, to examine the subject with great precision. Among the philosophers who have made researches on this head, our author confines his notice to Baumé, Briffon, Gouvenain, Struve, Jacob Fagot, of Stockholm, and Blagden. Of the results obtained by these philosophers, he gives concise tables, by means of which he has traced curves to exhibit the same with their comparative degrees of regularity and respective differences. See Plate XVIII.

In examining these curves, he found that those of Fagot and Struve abound with sinuosities, which prove that their experiments were not made with due care, and ought to be rejected out of the comparison: besides which, the fluids they used were too remote in their densities, from those which were subjected to experiment by the four other philosophers.

On examining the curves constructed from the experiments of the last, it was found that the curve of Baumé contains also a great number of sinuosities; that of Blagden less, and those of Briffon and Gouvenain were the most regular. These curves also appear to indicate, that the alcohol used by Briffon had a stronger attraction for water than that of Gouvenain; that this last was stronger in this respect than that of Blagden: and lastly, that the spirit used by Baumé had a weaker attraction than either of the other three. This important observation shews, that the practical use of any set of tables in determining the specific gravities of different mixtures of spirit and water, must, to a certain degree, be confined to that species of liquor from which the tables, or equivalent instrument was formed.

Notwithstanding the regularity of the tables of Briffon and Gouvenain, Cit. Haßenfratz was obliged to reject them in his farther proceedings, because those of Briffon were made at one temperature only; and from the very uniform course of the differences in those of Gouvenain, he saw reason to conclude, that they must have been made by interpolation between few and distant experiments. He gives, nevertheless, tabulated abstracts of their results. The Royal Society's experiments published by Sir Charles Blagden, are consequently the foundation of his construction of an alcograde.

If from these experiments curves be traced, expressing the densities of the several combinations of water and alcohol taken at different temperatures, he observes, that with regard to alcohol and its several combinations, as low as nine parts of alcohol, and one of water, the density follows the inverse proportion of the temperature, and the curve becomes a strait line; but that every less proportion of alcohol follows a different inverse proportion; that is to say, the line which passes through the extremities of the ordinates, is a curve of which the radius at curvature is longer the greater the proportion of water.

After some discussion of the method of forming the combinations by weight and by measure, the first of which has the great advantages of accuracy and facility of experiment, with regard to those operations which require change of temperature; and the latter possesses that of being more accommodated to the commercial habits of society; Citizen H. makes some observations on the difference between brandies, and the mixtures of alcohol and water. Brandy, says he, is the product obtained by distillation from a vinous liquor, in which the action of fire changes the order of the component parts, and carries over this fluid: alcohol is the product of a second operation of the same nature, in which similar effects are produced upon the brandy. In proof of the force of this method of considering the subject, he remarks, that the brandy will not again form wine by mixing it with the residue left in the still: neither will the alcohol form brandy by mixture with the residue, from which it was produced and driven over.

The differences of the vinous fluids in density above or below that of water, in flavor, and other obvious characters, are extreme; and the brandies from wine properly so called, from beer, peirry, cyder, &c. are no less remarkable for their peculiar qualities. All these afford alcohol, which has been supposed to be then the same fluid; but our author considers these also as differing in their immediate and intrinsic qualities. The alcograde is insufficient for the strict exhibition of the variations of these with water, and most obviously with respect to brandies, of which the price is governed so much by qualities and circumstances, not at all commensurate with their specific gravities. For wines, beer, and other immediate products of fermentation, this method of examination does not appear to be of any use.

He admits, nevertheless, that the alcograde is best adapted of any instrument for giving that approximate indication of value which the transactions of life demand. He proposes the construction of a floating instrument, which shall shew the proportion of alcohol by inspection at any known temperature by graduation. The following table, is, derived



rived from the Experiments of Gilpin, in the Philosophical Transactions 1794; but I must, for the sake of brevity, leave the consideration of the method of its fabrication to the scientific reader.

*Table of the lengths of the Tubes of Alcogrades, which shall shew the Proportions by measure of Alcohol in 1000 parts of any Mixture, at different Temperatures.*

Proportion of alcohol in 1000 parts of the whole mixture.	Length of the Tube for the Temperatures Centigrade:						
	0°	5°	10°	15°	20°	25°	30°
0	—9	—10	—7	0	+9	+21	+36
100	+102	+102	+113	118	130	141	152
200	180	182	197	205	220	243	257
300	240	260	279	300	328	344	362
400	327	356	424	427	442	468	493
500	446	482	515	552	588	620	660
520	484	516	559	587	624	658	698
540	508	551	585	623	661	697	737
560	550	587	622	660	699	737	777
580	589	624	660	698	739	778	819
600	630	662	700	738	780	820	862
620	670	702	742	779	823	863	906
640	711	743	786	822	868	908	952
660	753	785	831	866	914	954	1001
680	796	828	877	912	963	1001	1052
700	839	878	925	961	1012	1050	1104
720	885	921	976	1006	1062	1101	1157
740	933	970	1028	1061	1113	1154	1211
760	983	1021	1081	1127	1165	1209	1266
780	1036	1075	1135	1178	1217	1267	1322
800	1091	1130	1190	1231	1270	1327	1379
820	1147	1188	1247	1288	1327	1388	1437
840	1205	1248	1305	1348	1388	1450	1496
860	1266	1310	1364	1411	1451	1513	1556
880	1327	1375	1424	1477	1518	1577	1618
900	1390	1441	1485	1545	1588	1641	1682
920	1456	1507	1550	1616	1660	1709	1751
940	1524	1578	1620	1690	1734	1781	1826
960	1595	1652	1696	1767	1809	1858	1907
980	1669	1729	1778	1847	1885	1941	1994
1000	1741	1810	1864	1930	1961	2025	2086

In applying this table to the purpose of graduating the (cylindrical or prismical) stem of an alcograde or hydrometer, the author supposes the instrument to be plunged in a liquid of the specific gravity 90,000, and afterwards in another liquid \* of the specific gravity 100,000

\* In this process, since these divisions indicate ten thousandth parts of the portion most deeply immersed, and there are 2096 divisions on the whole stem, it will constitute rather more than one-fifth part of the bulk of

100,000, and that the difference or interval upon the stem between the intersection by the surface of the fluid in the first and in the last situation, shall be taken as a scale of 1000 parts; and of these parts the requisite portions being set off from the table upon the stem, as marked Fig. 1. Pl. XVIII. the instrument is then ready for use.

It seems almost unnecessary to remark, that the scale upon the stem must be read upon that vertical line which is marked with the temperature which a thermometer shews the fluid to possess. The inclined cross line, which intersects at the same place that vertical line and the surface of the fluid, is marked with the number denoting the proportion of alcohol contained in 1000 parts of the fluid. The reduction of this result to any desired strength or proof, of which the proportions of alcohol and water are known, will be easy to most arithmeticians, but would extend our limits too far if examples were to be given here.

of the instrument. Hence by a rough calculation, if the length of the stem be about five times the diameter of the ball, the diameter of the stem will prove about one-sixth part of the diameter of the ball.

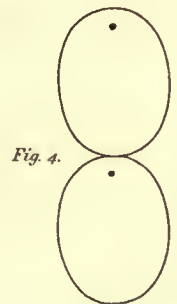
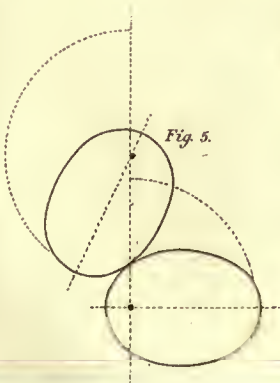
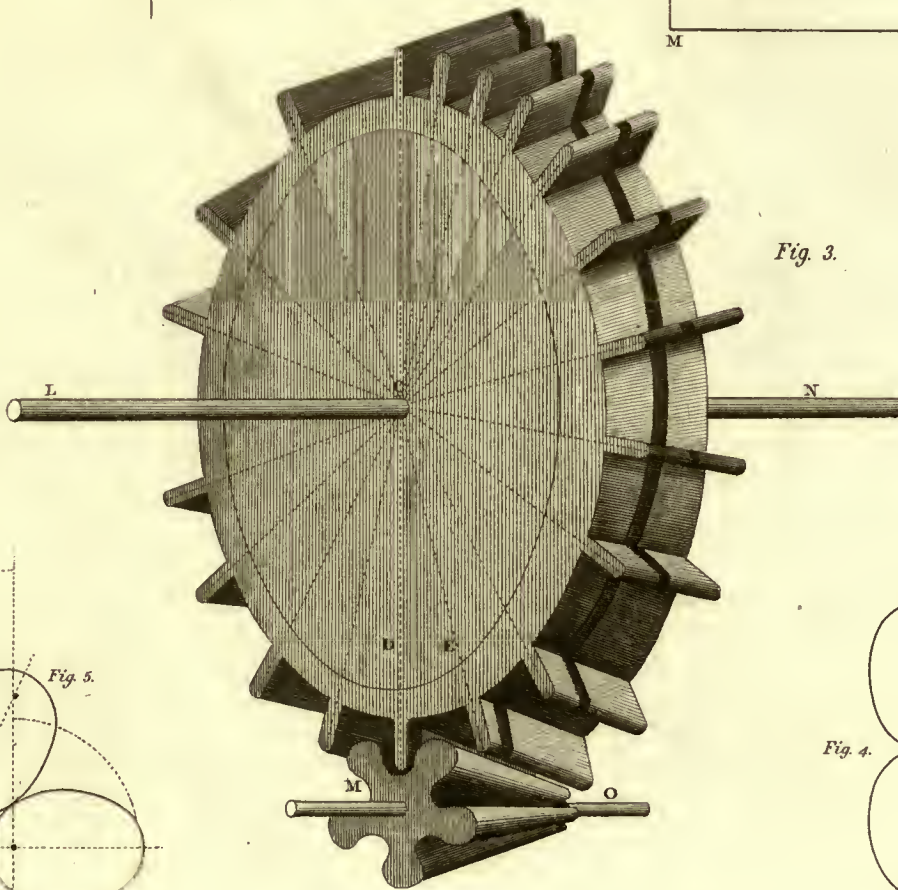
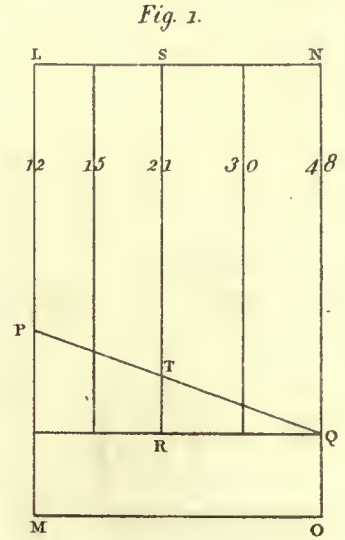
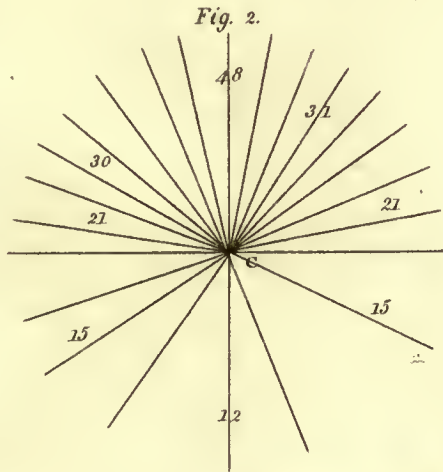
It is not necessary to prepare two fluids of the precise densities above-mentioned in order to find the interval, as it may easily be had from any two fluids whatever. Thus, if water at 1000, and alcohol at 840, were used, the interval on the stem would be denoted by  $\frac{1000-840}{1000}$  or  $\frac{160}{1000}$  or  $\frac{1600}{10000}$  of the portion

most deeply immersed, which are nearly the divisions of the table. Or, if a single fluid be preferred, then---

1. Weigh the whole instrument suspended to the dish of a pair of scales.
2. Find the apparent loss the instrument suffers by immersion in water, with a load or ballast attached to it, of which the residual weight is known, and can be allowed for. This immersion must be adjusted to the upper division of the stem.
3. Place a weight in the opposite scale, equal to one-fifth of that apparent loss. The instrument will rise, and the surface of the water will intersect a point at the distance of 2000 divisions below the first.
4. Or if any subdivision of that fifth part be put into the scale, the rise of the stem will be a proportionate part of 2000.---W. N.



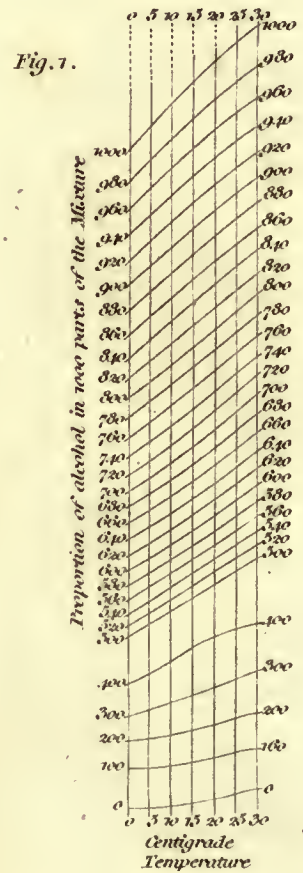
# *Wheel for shewing the Planetary motions.*







# Specific Gravity & Strength of ardent Spirit,







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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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JANUARY 1801.

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ARTICLE I.

*Construction and Use of an universal Table of Interest. By H. GOODWYN, Esq.*

TO MR. NICHOLSON.

SIR,

AS you have inserted other tables in your valuable periodical work, perhaps the enclosed may be thought worthy your notice.

It is not only applicable to the calculation of interest in this, but also in all other nations, and probably is as complete and concise as the nature of such a performance will admit. It is derived from that principle of prime numbers you have already honoured with publication.

I am,

SIR,

Your most humble servant,

H. GOODWYN.

East Smithfield, Dec. 15, 1800.

VOL. IV.—JANUARY 1801.

3 K

A Dc-

*A Description of the Table.*

This table consists of ten principal divisions.

In the left hand column of the first nine divisions are two cyphers, which in use have always the precedence.

The second column from the left of each of the first nine divisions contains at the top a cypher, followed by the nine digits, both arithmetically and perpendicularly arranged. And, either the cypher, or one of the digits, is always to be placed next to and on the left hand of the two cyphers first above-mentioned.

At the top of each of the first nine principal divisions is an horizontal line, containing eight circulating figures, which are to be annexed to the two first mentioned cyphers, and the other cypher, or digit, next mentioned above, according to such an arrangement as will be hereafter described.

In the body of each of the first nine divisions, and opposite either to the cypher or to one of the nine digits under the arithmetical arrangement before-mentioned, are ten broken horizontal rows of days.

The 1st, or uppermost rows, against the cyphers, contain all the days from				1 to 36
2d,	do.	1	do.	37 to 72
3d,	do.	2	do.	74 to 109
4th,	do.	3	do.	110 to 145
5th,	do.	4	do.	147 to 182
6th,	do.	5	do.	183 to 218
7th,	do.	6	do.	220 to 255
8th,	do.	7	do.	256 to 291
9th,	do.	8	do.	293 to 328
10th,	do.	9	do.	329 to 364

The tenth principal divisions contains five sets of days, with the interest of *1l. per cent. per annum*, placed against each of them respectively.

*Rules for applying the Table.*

Seek the number of days, for which the interest is required; in one of the first nine principal divisions, and these will be easily found by the arrangement above stated.

In the left hand column of the division adjoining the given number of days are two cyphers, with a decimal point on their left.

To these two cyphers add, from the next column, the cypher or single digit, which is placed in a line with the days first found.

Next to these cyphers or figures, and on their right, place the eight circulating figures, which are at the top of the division, beginning with that immediately over the given number of days (and which will be the first of the new circulating part of the complete interest)



and take out the whole eight in the order they stand, ending with that immediately on the left hand (which will be the last figure of the circulating part.) You will then have obtained the complete interest of 1*l.* at 1 *per cent.* for the given number of days.

To find the interest at 1 *per cent.* of the decuples of 1*l.* viz. 10*l.* 100*l.* 1000*l.* &c. move the decimal point one, two, three, &c. places more to the right hand, and the complete interest of the decuples will be obtained.

The interest of 1*l.* to its decuples at 1*l. per cent.* for any given number of days having been found, the interest of any other sum, at the same rate, may easily be derived from it by the rule of practice.

And this amount being obtained, let it be multiplied by any given rate of interest, and the product will be the complete interest of any given sum, for any given number of days at the given rate of interest.

### EXAMPLE I.

Required the interest of 1*l.* for 1 day, at the rate of 1 *per cent. per annum.*

In the first line of the first principal division is one day, and in the left hand column are two cyphers, viz.

In the second column from the left, and against the one day, is a cypher, viz.

In the line of circulates, immediately over the one day, is a cypher, viz.

And this is the first circulate of the complete interest.

The other seven circulates follow in rotation, thus

2739726

This sum is the answer

£. 00002732726

### EXAMPLE II.

Required the complete interest of 1*l.* at 1 *per cent. per annum* for 364 days.

By the foregoing part or description of the table 364 days must be in the tenth, or last, column of one of the first nine principal divisions; and accordingly it is found at the bottom of the first division.

As before, in the left hand column are two cyphers

In the second column from the left, and against the 364 days, is fig.

In the line of circulates immediately over 364 days, is fig.

The remaining circulates are

7260273

Answer

£. 00997260273

## EXAMPLE III.

Required the interest of 10*l.* 100*l.* and 1000*l.* for 1 day, at 1 per cent.

The interest of 1*l.* at 1 per cent. for 1 day, per example 1, is  $\text{£} \cdot 00002739726$

do.	10 <i>l.</i>	at	do.	$\cdot 0002739726$
do.	100 <i>l.</i>	at	do.	$\cdot 002739726$
do.	1000 <i>l.</i>	at	do.	$\cdot 02739726$

## EXAMPLE IV.

Required the interest of 1000000*l.* for 364 days at the rate of 1*l.* per cent. per annum.

By examples 2 and 3, applied properly, the answer will be  $9972 \cdot 60273 = \text{£} \cdot 9972 \cdot 12 \cdot 0 \frac{1}{2}$ .

## EXAMPLE V.

Required the interest of 1000000*l.* for 247 days at 5*l.* per cent.

The interest of 1000000*l.* for 247 days, at 1*l.* per

cent. is  $\text{£} 6767 \cdot 12328$

Answer  $\text{£} \cdot 33835 \cdot 61643 = \text{£} \cdot 33835 \ 12 \ 3$

## EXAMPLE VI.

Required the interest of 1220 at 4*l.* per cent. per annum for 263 days.

The interest of 1*l.* at 1*l.* per cent. per annum for

263 days is  $\cdot 00720547945$ , and of  $\text{£} \cdot 1000$  is  $\text{£} \cdot 7 \cdot 20547$

200 or  $\frac{2}{3}$   $1 \cdot 44109$

20 or  $\frac{1}{3}$   $\cdot 14416$

$\text{£} \cdot 8 \cdot 79066$

4

Answer  $\text{£} \cdot 35 \cdot 1624 = \text{£} \cdot 35 \ 3 \ 3$



## A TABLE

Shewing, by Inspection, the complete Interest of One Pound at the Rate of  $\text{£} .1$  per cent. per annum; and by Induction the complete Interest of any Sum, at any Rate, and for any given Number of Days, from 1 to 365, in Decimals of a Pound Sterling.

0 2 7 3 9 7 2 6									
Days.									
0	1	10	27						22
1				51	72	63	46		
2	74	83	100						95
3				124	145	136	119		
4	147	156	173						168
5				197	218	209	192		
6	220	229	246						241
7				270	291	282	265		
8	293	302	319						314
9				343	364	355	338		

1 0 9 5 8 9 0 4									
Days.									
0	4		35			33			15
1		40		58	69		38		
2	77		108			106			88
3			113		131	142		111	
4	150		181			179			161
5		186		204	215		184		
6	223		254		252				234
7		259		277	288		257		
8	296		327			325			307
9		332		350	361		330		

2 4 6 5 7 5 3 4									
Days.									
0	9	17	24	21					
1					64	56	49	52	
2	82	90	97	94					
3					137	129	122	125	
4	155	163	170	167					
5					210	202	195	198	
6	228	236	243	240					
7					283	275	268	271	
8	301	309	316	313					
9					356	341	348	344	

0 5 4 7 9 4 5 2									
Days.									
0	2	20		29					19
1		54		71	53				44
2	75	93		102					92
3			127		144	126			117
4	148	166		175					165
5		200		217	199				190
6	221	239		248					238
7		273		290	272				263
8	294	312		321					311
9		346		363	345				336

1 3 6 9 8 6 3 0									
Days.									
0	5		36			23			11
1		50	62		68				37
2	78		109			96		84	
3		123	135		141				110
4	151		182			169		157	
5		196	208		214				183
6	224		255		242	230			
7		269	281		287				256
8	297		328			315		303	
9		342	354		360				329

3 2 8 7 6 7 1 2									
Days.									
0	12		32	28			26		
1		47			61			41	45
2	85		105		101			99	
3		120			134				114
4	158		178		174			172	
5		193			207				187
6	231		251		247			245	
7		266			280				260
8	304		324		320			318	
9		339			353				333

0 8 2 1 9 1 7 8									
Days.									
0	3	30	8		7				
1				70	43	65	66		
2	76	103	81	80					
3				143	116	138	139		
4	149	176	154	153					
5				216	189	211	212		
6	222	249	227	226					
7				289	262	284	285		
8	295	322	300	299					
9				362	335	357	358		

1 6 4 3 8 3 5 6									
Days.									
0	6		16		14			13	
1		60			67			57	59
2	79		89		87			86	
3		133			140			130	132
4	152		162		160			159	
5		206			213			203	205
6	225		235		233			232	
7		279			286			276	278
8	298		308		306			305	
9		352			359			349	351

4 9 3 1 5 0 6 8									
Days.									
0	18	34						25	31
1		48	42	55	39				
2	91	107						98	104
3		121	115	128	112				
4	164	180						171	177
5		194	188	201	185				
6	237	253						244	250
7		267	261	274	251				
8	310	326						317	323
9		340	334	347	331				

Days.	Interest.	Days.	Interest.	Days.	Interest.	Days.	Interest.	Days.	Interest.
73	£. 002	146	£. 004	219	£. 006	292	£. 008	365	£. 01

## II.

*A Chemical Analysis of Three Species of Whinstone, and Two of Lava.* By ROBERT KENNEDY, M. D. F. R. S. & F. A. S. Edin.

(Concluded from page 415.)

## ANALYSIS III.

*Whin of the Calton Hill, near Edinburgh.*

THE rock of this hill varies much in different parts; but its general character is that of porphyry. The piece I chose for analysis was taken from the fourth side, about ten or twelve feet below its highest point; and was free from calcareous spar.

The external characters of this particular piece are as follow: it consists of a greyish basis, containing rhomboidal crystals of felspar of a light reddish-brown colour; and small spherical masses of green earth. The basis, in its fracture, is uneven, and earthy, and has no lustre. It can be scratched easily with a knife, and gives an earthy smell when breathed on. The green earth is soft, and is affected in some degree by water; and being decomposed by the weather, as well as the veins and nodules of calcareous spar, which are very common in this hill, the rock in many places is extremely porous.

The specimen I have described may be called argillaceous porphyry. It effervesces slightly with acids; so that the lime which it contains must be united to carbonic acid. Its powder is light grey, with a certain shade of purple. When heated to redness, it becomes of a brown colour. It is not attracted by the magnet, either in its natural state, or after ignition. By being exposed to a low red heat for half an hour, it loses 5 *per cent.* of its weight. It softens at 44 of Wedgwood. Its specific gravity is 2.663, as nearly as I could ascertain, from the effect which the water had in making it crumble down.

One hundred parts contain,

Silex,	-	-	-	-	-	-	-	50
Argil,	-	-	-	-	-	-	-	18.50
Oxyde of iron,	-	-	-	-	-	-	-	16.75
Carbonate of Lime,	-	-	-	-	-	-	-	3
Moisture, and other vol. matter,	-	-	-	-	-	-	-	5
Soda, about	-	-	-	-	-	-	-	4
Muriatic acid, about	-	-	-	-	-	-	-	1

---

98.25

ANALYSIS



# ANALYSIS IV.

## *Lava of Catania. Ætna.*

This lava, and the species to be next mentioned, were brought from Mount Ætna by Sir James Hall and Dr. James Home. At their request I analyzed specimens of each. Mineralogists are well acquainted with these lavas, from the descriptions which have been given of them by M. Dolomieu; therefore it is unnecessary for me to mention their external characters.

The lava of Catania gives a powder of a light grey colour, which is very little changed in appearance by being heated red hot. After it is wet, it becomes dark grey. When this lava is reduced to small fragments, some parts of it are attracted by the magnet, and others are not. In fine powder it is but feebly attracted; and after ignition its qualities in this respect do not seem to be altered. It softens at 33 of Wedgwood. The specific gravity of the pieces most free from air bubbles, is 2.795.

I have exposed this lava, in the manner already described, to various degrees of heat, from redness to 158 of Wedgwood, and constantly observed that it never lost the smallest weight.

When boiled like the whins in muriatic acid, the part remaining undissolved weighed 68 per cent.

There are in 100 parts,

Silex,	51
Argil,	19
Oxyde of iron,	14.50
Lime,	9.50
Soda, about	4
Muriatic acid, about	1
	<hr/>
	99.

# ANALYSIS V.

## *Lava S<sup>ta</sup> Venere, Piedimonte, Ætna\*.*

This species gives, like the preceding, a greyish powder, which becomes dark grey when wet, and its colour is scarcely affected by being exposed to a low red heat. It is but feebly attracted by the magnet, whether in small fragments or in powder; and its qualities in this respect are not changed by low ignition.

\* Dolomieu, in describing this lava, says, that its fracture is conchoidal, like that of silex. The specimen which I analyzed had an uneven fracture; and its colour was blackish-blue: in other respects, however, it answered to Dolomieu's description.

It softens at 32 of Wedgwood, and does not lose any weight in fires between 150 and 160. After being boiled in muriatic acid, it left 68 parts *per cent.* undissolved. Its specific gravity is 2.823.

In 100 parts I found,

Silex,	-	-	-	-	-	-	-	-	50.75
Argil,	-	-	-	-	-	-	-	-	17.50
Oxyde of iron,	-	-	-	-	-	-	-	-	14.25
Lime,	-	-	-	-	-	-	-	-	10
Soda, about	-	-	-	-	-	-	-	-	4
Muriatic acid, about	-	-	-	-	-	-	-	-	1

---

97.50

In analyzing these two lavas, I examined the different solutions, with particular attention, for magnesia, and sulphuric acid; but could not detect any traces of either of these substances.

The results of these analyses show, that whins, and a certain class of lavas, taken from remote quarters of the globe, consist of the same component elements, united in each, nearly in the same proportion. The only circumstance in which they materially differ, is the loss of some volatile matter in the fire, which is peculiar to the whins alone.

We need not be now surprised at the facts mentioned by Dolomieu, and others, of soda being found about volcanos, or upon the surface of lavas; as it has thus been shown to exist in these substances in combination with their earthy bases.

---

The facts and experiments I am next to mention will prove, that whins and lavas are not the only stones which contain soda; and will even render it probable, that this alkali is widely diffused through mineral bodies,

Soon after I first discovered it in whins, and had communicated the circumstance to Sir James Hall, he sent me, from a high sandstone rock in his estate, a quantity of the stone decomposed; and informed me that there was a saline efflorescence mixed with it, which was collected along with the loose matter, and which seemed, by the taste, to be sea-salt. The place has been long called by the common people the *Salt Heugh*. By simply boiling some of the decomposed sandy part in water, and afterwards filtering and evaporating the water, I obtained regular crystals of sea-salt, mixed with a small quantity of sulphate of soda.

Hence it appeared likely, that common salt would be found to be one of the component parts of ordinary sandstone strata. To verify this important observation, I next examined two hard and solid specimens, taken from some depth below the surface, and perfectly free from decomposition. The first was broken from a quarry about two miles to the westward



of Edinburgh. A portion of it being reduced to minute grains, of such a size as the particles of the stone seemed to have consisted of originally, was mixed with some diluted nitric acid, and boiled with it gently for three hours. The acid, after being filtered, was examined with nitrate of barytes, with which it produced only a slight cloud. But nitrate of silver, when poured into it, threw down a copious white precipitate of muriate of silver, indicating the presence of muriatic acid.

After this precipitate was separated by filtration, the liquor which passed through was evaporated to dryness. A saline matter remained, which being mixed with some charcoal, and heated, deflagrated like nitrous salts. Having washed the coaly residuum, and evaporated the water, I got some perfectly pure carbonate of soda. There had been, therefore, in the sandstone, some common salt, which, by this process, was decomposed, and its acid and alkali collected separately; but whether the whole of the salt was obtained, and what proportion it bore to the earthy parts, I cannot determine, as the stone itself was not analyzed.

The next specimen was taken from a stratum of sandstone, which lies below the hill of Salisbury Craig; and I chose this species, because the whin to which it is contiguous has already been shown to contain soda. Some of it being reduced to the state of fine sand, and treated in every respect as the preceding, gave a portion of muriate of silver, and of carbonate of soda. The existence, therefore, of sea-salt in these varieties of sandstone, is thus fully established\*.

The celebrated Mr. Klaproth of Berlin has already shown, that pot-ash enters into the composition of several stony substances; and by the experiments described in this paper, the other fixed alkali, soda, has also been proved to exist in mineral bodies, as it has been separated from nine different varieties; all of which also contain a certain quantity of muriatic acid.

As caustic fixed alkali was much used in these analyses, I shall conclude this paper by describing, in a few words, the method by which I prepare it; both because its purity is of the greatest importance, and because the process I employ differs, in some circumstances, from that of most chemists. Having obtained an alkali free from all earthy matter, either by burning white tartar, or by repeated solutions and crystallizations of carbonate of soda, I dissolve it in a considerable quantity of water. The requisite proportion of lime being slacked, and allowed to cool, it is diluted with water, and then mixed with the solution of the alkali. This mixture is frequently stirred during two or three days; for when no heat is applied, the lime requires a certain time to attract the whole of the carbonic acid. In the next place, the mixture is filtered through a piece of linen placed in a funnel, and water poured on it till the whole of the alkali is washed out. As it passes through, it is poured, at intervals, from the bottle which receives it first, into a second that is closely stopped.

\* Since these experiments were performed, I have seen several decomposed sandstones, on the surfaces of which there was an efflorescence of common salt.

In making the fixed alkalis caustic, it is usual to boil them with the lime; but as most kinds of limestone contain a small quantity of siliceous earth or argil, and as these earths, when in a state of division, are soluble in boiling caustic alkali, there is a probability, when heat is applied, of its being thus rendered impure. This is my reason for carefully avoiding heat in the first part of the process; and I have not found that lime made from chalk, or from the purer kinds of limestone \*, give any impurity whatever to the alkali, when mixed with it cold.

The solution, in its weak state, is first evaporated in a basin of hammered iron, polished; but when it is somewhat concentrated, I carry on the evaporation in a dish made of the purest silver, reduced from *luna cornea*. After being boiled to a small quantity, it is allowed to cool, and then put into a well-stopped bottle for some days; during which, if the evaporation has been continued long enough, the neutral salts crystallize, as Mr. Lowitz has pointed out. Afterwards, the solution is carefully decanted from these salts, and again evaporated in the silver dish, till it acquires the consistence of thin oil †. In this state, so little water is present, that any part of the alkali, which may be united to carbonic acid, crystallizes, and also any neutral salt that may remain; and the solution being decanted a second time, is obtained perfectly pure, colourless, and transparent.

In the last boiling it is somewhat difficult to observe the exact degree of concentration at which all the alkaline carbonate will crystallize; and if the evaporation is carried too far, the caustic alkali, if pot-ash, crystallizes itself: so that several evaporations are sometimes requisite.

When no more water remains in the solution than is just sufficient to hold the caustic alkali dissolved, it contains nearly half its weight of alkali; but the exact quantity is easily known, by evaporating a portion to dryness in a silver crucible. Before using such a solution for analyses, I ascertain its purity in the following manner: some of it being supersaturated with perfectly pure nitric acid, is examined with nitrate of barytes and of silver; with neither of which, if properly made, it will give the smallest cloud; consequently it can contain no sulphuric or muriatic acid.

Another portion being saturated exactly with a pure acid, the whole is evaporated to dryness, and the salt left is redissolved in a little water. If any earth were contained in the caustic alkali, it would remain thus undissolved; but when made as above described, I have never, in this examination, observed the smallest sediment.

After the alkali is purified from neutral salts, and from the part united to carbonic acid, it may itself be crystallized by farther evaporation, as Mr. Lowitz has shown. But this process seems of no use in chemical analysis, as the alkali is previously obtained altogether pure.

\* Mr. Klaproth uses lime made of Carrara marble, which he boils with the alkali. (*Beitrage*, vol. i. preface.)

† Mr. Lowitz, in describing his process for crystallizing caustic pot-ash, directs the last evaporation to be performed in a glass retort. This method is very erroneous; as the alkali, when heated and concentrated, will dissolve large quantities of the glass,



## III.

*Description and Use of a cheap and simple Apparatus for teaching the first Principles of Mechanics: By RICHARD LOVELL EDGEWORTH, Esq. F. R. S. and M. R. I. A.\**

WE do not mean to undervalue either the application of strict demonstration to problems in mechanics, or the exhibition of the most accurate machinery in philosophical lectures; but we wish to point out a method of giving a general notion of the mechanical organs to our pupils, which shall be immediately obvious to their comprehension, and which may serve as a sure foundation for future improvement. When a person perceives the effect of his own bodily exertions with different engines, and when he can compare in a rough manner their relative advantages, he is not disposed to reject their assistance, or to expect more than is reasonable from their application. The young theorist in mechanics thinks he can produce a perpetual motion! When he has been accustomed to refer to the plain dictates of common sense and experience, on this, as well as on every other subject, he will not easily be led astray by visionary theories.

To bring the sense of feeling to our assistance in teaching the uses of the mechanic powers, the following apparatus was constructed, to which we have given the name Panorganon.

It is composed of two principal parts; a frame to contain the moving machinery; and a *capstan* or *windlass*, which is erected on a *floor* plank, that is sunk a few inches into the ground; the frame is by this means and by six braces or props rendered steady. The cross rail, or *transom*, is strengthened by braces and a king-post to make it lighter and cheaper. The *capstan* consists of an upright shaft, upon which are fixed two *drums*; about which a rope may be wound up, and two levers or arms by which it may be turned round. There is also a screw of iron coiled round the lower part of the shaft, to shew the properties of the screw as a mechanic power. The rope which goes round the *drum* passes over one of the pulleys near the top of the frame, and under another pulley near the bottom of the frame. As two *drums* of different sizes are employed, it is necessary to have an upright roller to conduct the rope in a proper direction to the pulleys, when either of the *drums* is used. Near the frame, and in the direction in which the rope runs, is laid a platform or road of deal boards, one board in breadth, and twenty or thirty feet long, upon which a small sledge loaded with different weights may be drawn. Plate XIX. Fig. 1.

F. F. The frame. b. b. Braces to keep the frame steady. a. a. a. Angular braces to strengthen the transom; and also a *king-post*. S. A round, taper, shaft, strengthened above and below the mortices with iron hoops. L. L. Two arms or levers by which the

\* Extracted by permission from "Practical Education by Maria Edgeworth, and Richard Lovell Edgeworth, F. R. S. and M. R. I. A."

shaft, &c. are to be moved round. D D. The drum, which has two rims of different circumferences. R. The roller to conduct the rope. P. The pulley, round which the rope passes to the larger drum. P 2. Another pulley to answer to the smaller drum. P 3. A pulley through which the rope passes when experiments are tried with levers, &c. P 4. Another pulley through which the rope passes when the sledge is used. Ro. The road of deal boards for the sledge to move on. Sl. The sledge with pieces of hard wood attached to it to guide it on the road.

### *Uses of the Panorganon.*

As this machine is to be moved by the force of men or children, and as their force varies not only with the strength and weight of each individual, but also according to the different manner in which that strength or weight is applied, it is, in the first place, requisite to establish one determinate mode of applying human force to the machine; and also a method of determining the relative force of each individual whose strength is applied to it.

*To estimate the Force with which a Person can draw horizontally by a Rope over his Shoulder.*

*Experiment 1.* Hang a common long scale-beam (without scales or chains) from the top or *transom* of the frame, so as that one end of it may come within an inch of one side or post of the machine. Tie a rope to the hook of the scale-beam, where the chains of the scale are usually hung, and pass it through the pulley P 3, which is about four feet from the ground; let the person pull this rope from 1 towards 2, turning his back to the machine, and pulling the rope over his shoulder, Fig. 6. As the pulley may be either too high or too low to permit the rope to be horizontal, the person who pulls it should be placed ten or fifteen feet from the machine, which will lessen the angular direction of the cord, and the inaccuracy of the experiment. Hang weights to the other end of the scale-beam, till the person who pulls can but just walk forward, pulling fairly without propping his feet against any thing. This weight will estimate the force with which he can draw horizontally by a rope over his shoulder\*. Let a child who tries this walk on the board with dry shoes; let him afterwards chalk his shoes, and afterwards try it with his shoes soaped: he will find that he can pull with different degrees of force in these different circumstances; but when he tries the following, let his shoes be always dry, that his force may be always the same.

### *To shew the Power of the three different Sorts of Levers.*

*Experiment 2.* Instead of putting the cord that comes from the scale-beam, as in the last experiment, over the shoulder of the boy, hook it to the end 1 of the lever L, Fig. 2. This lever is passed through a socket, Fig. 3. in which it can be shifted from one of its

\* Were it thought necessary to make these experiments perfectly accurate, a segment of a pulley, the radius of which is half the length of the scale-beam, should be attached to the end of the beam; upon which the cord may apply itself, and the pulley (P 3.) should be raised or lowered, to bring the rope horizontally from the man's shoulder when in the attitude of drawing.



ends towards the other ; and can be fastened at any place by the screw of the socket. This socket has two gudgeons, upon which it, and the lever which it contains can turn. This socket and its gudgeons can be lifted out of the holes in which it plays, between the rail R R, Plate XIX. Fig. 2. and may be put into other holes at R R, Fig. 5. Loop another rope to the other end of this lever, and let the boy pull as before. Perhaps it should be pointed out, that the boy must walk in a direction contrary to that in which he walked before ; viz. from 1 towards 3. The height to which the weight ascends, and the distance to which the boy advances, should be carefully marked and measured ; and it will be found, that he can raise the weight to the same height, advancing through the same space as in the former experiment. In this case, as both ends of the lever moved through equal spaces, the lever only changed the direction of the motion, and added no mechanical power to the direct strength of the boy.

*Experiment 3.* Shift the lever to its extremity in the socket ; the middle of the lever will be now opposite to the pulley, Fig. 4 ; hook to it the rope that goes through the pulley P 3, and fasten to the other end of the lever the rope by which the boy is to pull. This will be a lever of the second kind, as it is called in books of mechanics ; in using which, the resistance is placed between the centre of motion or fulcrum, and the moving power. He will now raise double the weight that he did in Experiment II. and he will advance through double the space.

*Experiment 4.* Shift the lever, and the socket which forms the axis, (without shifting the lever from the place in which it was in the socket in the last experiment) to the holes that are prepared for it at R R, Fig. 5. The free end of the lever E will now be opposite to the rope, and to the pulley (over which the rope comes from the scale-beam). Hook this rope to it, and hook the rope by which the boy pulls to the middle of the lever. The effect will now be different from what it was in the two last experiments ; the boy will advance only half as far, and will raise only half as much weight as before. This is called a lever of the third sort. The first and second kinds of levers are used in quarrying ; and the operations of many tools may be referred to them. The third kind of lever is employed but seldom, but its properties may be observed with advantage whilst a long ladder is raised, as the man who raises it is obliged to exert an increasing force till the ladder is nearly perpendicular. When this lever is used, it is obvious, from what has been said, that the power must always pass through less space than the thing which is to be moved ; it can never, therefore, be of service in gaining power. But the object of some machines is to increase velocity, instead of obtaining power, as in a sledge-hammer moved by mill-work. (V. the plates in Emerson's Mechanics, No. 236.)

The experiments upon levers may be varied at pleasure, increasing or diminishing the mechanical advantage, so as to balance the power and the resistance, to accustom the learners to calculate the relation between the power and the effect in different circumstances ; always pointing out, that whatever excess there is in the power\*, or

\* The word *power* is here used in a popular sense, to denote the strength or efficacy that is employed to produce an effect by means of any engine.

in the resistance, is always compensated by the difference of space through which the inferior passes.

The experiments which we have mentioned are sufficiently satisfactory to a pupil, as to the immediate relation between the power and the resistance; but the different spaces, through which the power and the resistance move when one exceeds the other, cannot be obvious, unless they pass through much larger spaces than levers will permit.

*Experiment 5.* Place the sledge on the farthest end of the wooden road, Fig. 1.; fasten a rope to the sledge, and conduct it through the lowest pulley P 4, and through the pulley P 3, so as that the boy may be enabled to draw it by the rope passed over his shoulder. The sledge must now be loaded, till the boy can but just advance with short steps steadily upon the wooden road; this must be done with care, as there will be but just room for him beside the rope. He will meet the sledge exactly on the middle of the road, from which he must step aside to pass the sledge. Let the time of this experiment be noted. It is obvious that the boy and the sledge move with equal velocity, there is therefore no mechanical advantage obtained by the pulleys. The weight that he can draw will be about half a hundred, if he weigh about 9 stone; but the exact force with which the boy draws is to be known by Experiment 1.

#### *The Wheel and Axle:*

This organ is usually called in mechanics, *The axis in peritrochio*. A hard name, which might well be spared, as the word windlass or capstan would convey a more distinct idea to our pupils.

*Experiment 6.* To the largest drum, Fig. 1. fasten a cord, and pass it through the pulley P downwards, and then through the pulley P 4 to the sledge placed at the end of the wooden road, which is farthest from the machine. Let the boy, by a rope fastened to the extremity of one of the arms of the capstan, and passed over his shoulder, draw the capstan round; he will wind the rope round the drum, and draw the sledge upon its road. To make the sledge advance twenty-four feet upon its road, the boy must have walked circularly 144 feet, which is six times as far, and he will be able to draw about three hundred weight, which is six times as much as in the last experiment.

It may now be pointed out, that the difference of space, passed through by the power in this experiment, is exactly equal to the difference of weight, which the boy could draw without the capstan.

*Experiment 7.* Let the rope be now attached to the smaller drum; the boy will draw nearly twice as much weight upon the sledge as before, and will go through double the space.

*Experiment 8.* Where there are a number of boys, let five or six of them, whose power of drawing (estimated as in Experiment 1.) amounts to six times as much as the force of the boy at the capstan, pull at the end of the rope which *was* fastened to the sledge; they will balance the force of the boy at the capstan: either they, or he, by a sudden pull may advance,



advance, but if they pull fairly, there will be no advantage on either part. In this experiment the rope should pass through the pulley P 3, and should be coiled round the larger drum. And it must be also observed, that in all experiments upon the motion of bodies, in which there is much friction, as where a sledge is employed, the results are never so uniform as in other circumstances.

### *The Pulley.*

Upon the pulley we shall say little, as it is in every body's hands, and experiments may be tried upon it without any particular apparatus. It should, however, be distinctly inculcated, that the power is not increased by a fixed pulley. For this purpose, a wheel without a rim, or, to speak with more propriety, a number of spokes fixed in a nave should be employed, (Fig. 9.) Pieces like the heads of crutches should be fixed at the ends of these spokes, to receive a piece of girth-web, which is used instead of a cord, because a cord would be unsteady; and a strap of iron with a hook to it should play upon the center, by which it may at times be suspended, and from which at other times a weight may be hung.

*Experiment 9.* Let this skeleton of a pulley be hung by the iron strap from the transom of the frame; fasten a piece of web to one of the radii, and another to the end of the opposite radius. If two boys of equal weight pull these pieces of girth-web, they will balance each other; or two equal weights hung to these webs will be in equilibrio. If a piece of girth-web be put round the uppermost radius, two equal weights hung at the ends of it will remain immoveable; but if either of them be pulled, or if a small additional weight be added to either of them, it will descend, and the web will apply itself successively to the ascending radii, and will detach itself from those that are descending. If this movement be carefully considered, it will be perceived, that the web in unfolding itself, acts in the same manner upon the radii as two ropes would if they hung to the extremities of the opposite radii in succession. The two radii which are opposite, may be considered as a lever of the first sort, where the center is in the middle of the lever; as each end moves through an equal space, there is no mechanical advantage. But if this skeleton-pulley be employed as a common *block* or *tackle*, its motions and properties will be entirely different.

*Experiment 10.* Fig. 9. Nail a piece of girth-web to a post, at the distance of three or four feet from the ground; fasten the other end of it to one of the radii. Fasten another piece of web to the opposite radius, and let a boy hold the skeleton-pulley suspended by the web; hook weights to the strap that hangs from the center. The end of the radius to which the fixed girth-web is fastened will remain immoveable; but, if the boy pulls the web which he holds in his hand upwards, he will be able to lift nearly double the weight, which he can raise from the ground by a simple rope, without the machine, and he will perceive that his hand moves through twice as great a space as the weight ascends: he has therefore the mechanical advantage, which he would have by a lever of the second sort, as  
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in experiment III. Let a piece of web be put round the under radii, let one end of it be nailed to the post, and the other be held by the boy, and it will represent the application of a rope to a moveable pulley; if its motion be carefully considered, it will appear that the radii, as they successively apply themselves to the web, represent a series of levers of the second kind. A pulley is nothing more than an infinite number of such levers; the cord at one end of the diameter serving as a fulcrum for the *organ* during its progress. If this *skeleton-pulley* be used horizontally instead of perpendicularly, the circumstances which have been mentioned will appear more obvious.

Upon the wooden road lay down a piece of girth-web; nail one end of it to the road; place the pulley upon the web at the other end of the board, and bringing the web over the radii, let the boy, taking hold of it, draw the loaded sledge fastened to the hook at the center of the pulley: he will draw nearly twice as much in this manner as he could without the pulley\*.

Here the web lying on the road shews more distinctly, that it is quiescent where the lowest radius touches it; and if the radii, as they tread upon it, are observed, their points will appear at rest, whilst the center of the pulley will go as fast as the sledge, and the top of each radius successively (and the boy's hand which unfolds the web) will move twice as fast as the center of the pulley and the sledge.

If a person, holding a stick in his hand, observes the relative motions of the top, and the middle, and the bottom of the stick, whilst he inclines it, he will see that the bottom of the stick has no motion on the ground, and that the middle has only half the motion of the top. This property of the pulley has been dwelt upon, because it elucidates the motion of a wheel rolling upon the ground; and it explains a common paradox, which appears at first inexplicable, "The bottom of a rolling wheel never moves *upon* the road." This is asserted only of a wheel moving over hard ground, which, in fact, may be considered rather as laying down its circumference upon the road, than as moving upon it.

#### *The inclined Plane and the Wedge.*

The *inclined plane* is to be next considered. When a heavy body is to be raised, it is often convenient to lay a sloping artificial road of planks, up which it may be pushed or drawn. This mechanical power, however, is but of little service without the assistance of wheels or rollers: we shall therefore speak of it as it is applied in another manner, under the name of *the wedge*, which is in fact a moving inclined plane; but if it is required to explain the properties of the inclined plane by the *Panorganon*, the wooden road may be raised and set to any inclination that is required, and the sledge may be drawn upon it as in the former experiments.

\* In all these experiments with the skeleton-pulley somebody must keep it in its proper direction; as from its structure, which is contrived for illustration, not for practical use, it cannot retain its proper situation without assistance.



Let one end of a lever, Fig. 7. with a wheel at one end of it, be hinged to the post of the frame, by means of a gudgeon driven or screwed into the post. To prevent this lever from deviating sideways, let a slip of wood be connected with it by a nail, which shall be fast in the lever, but which moves freely in a hole in the rail. The other end of this slip must be fastened to a stake driven into the ground at three or four feet from the lever, at one side of it, and towards the end in which the wheel is fixed, Fig. 10. which is a *vue d'oiseau*, in the same manner as the treadle of a common lathe is managed, and as the treadle of a loom is sometimes guided\*.

*Experiment 11.* Under the wheel of this lever place an inclined plane or half-wedge, Fig. 7. on the wooden road, with rollers under it, to prevent friction†; fasten a rope to the foremost end of the wedge, and pass it through the pulleys (P. 4. and P. 3.) as in the fifth experiment. Let a boy draw the sledge by this rope over his shoulder, and he will find, that as it advances it will raise the weight upwards; the wedge is five feet long, and elevated one foot. Now, if the perpendicular ascent of the weight, and the space through which he advances be compared, he will find that the space through which he has passed will be five times as great as that through which the weight has ascended; and that *this* wedge has enabled him to raise five times as much as he could raise without it, if his strength were applied, as in Experiment I, without any mechanical advantage. By making this wedge in two parts hinged together, with a graduated piece to keep them asunder, the wedge may be adjusted to any given obliquity; and it will be always found, that the mechanical advantage of the wedge may be ascertained by comparing its perpendicular elevation with its base. If the base of the wedge is 2, 3, 4, 5, or any other number of times greater than its height, it will enable the boy to raise respectively 2, 3, 4, or 5 times more weight than he could do in Experiment I, by which his power is estimated.

#### The Screw.

*The screw* is an inclined plane wound round a cylinder; the height of all its revolutions round the cylinder taken together, compared with the space, through which the power that turns it passes, is the measure of its *mechanical advantage*‡. Let the lever, used in the last experiment, be turned in such a manner as to reach from its gudgeon to the shaft of the Panorganon, guided by an attendant lever as before, Fig. 8. Let the wheel rest upon

\* In a loom this secondary lever is called a *lamb*, by mistake, for *lam*; from *lamina*, a slip of wood.

† There should be three rollers used; one of them must be placed before the sledge, under which it will easily find its place, if the bottom of the sledge near the foremost end is a little sloped upwards. To retain this foremost roller in its place till the sledge meets it, it should be stuck slightly on the road with two small bits of wax or pitch.

‡ *Mechanical advantage* is not a proper term, but our language is deficient in proper technical terms. The word *power* is used so indiscriminately, that it is scarcely possible to convey our meaning, without employing it more strictly.

the lowest *belix* or thread of the screw; as the arms of the shaft are turned round, the wheel will ascend, and carry up the weight which is fastened to the lever\*. As the situation of the screw prevents the weight from being suspended exactly from the center of the screw, proper allowance must be made for this in estimating the force of the screw, or determining the mechanical advantage gained by the lever: this can be done by measuring the perpendicular ascent of the weight, which in all cases is better, and more expeditious, than measuring the parts of a machine, and estimating its force by calculation; because the different diameters of ropes, and other small circumstances, are frequently mistaken in estimates.

The space passed through by the moving power, and by that which it moves, are infallible data for estimating the powers of engines. Two material subjects of experiments yet remain for the Panorganon; friction, and wheels of carriages. We repeat, that it is not intended in this, or in any other part of our design, to write treatises upon science; but merely to point out methods of initiating young people in the rudiments of knowledge, and of giving them a clear and distinct view of those principles upon which they are founded. No preceptor, who has had experience, will cavil at the superficial knowledge of a boy of twelve or thirteen upon these subjects; he will perceive, that the general view, which we wish to give our pupils of the useful arts and sciences, must certainly tend to form a taste for literature and investigation. The *sciologist* has learned only to *talk*—we wish to teach our pupils to *think*, upon the various objects of human speculation.

The Panorganon may be employed in trying the resistance of air and water; the force of different muscles; and in a great variety of amusing and useful experiments. In academies, and private families, it may be erected in the place allotted for amusement, where it will furnish entertainment for many a vacant hour. When it has lost its novelty, the shaft may from time to time be taken down, and a swing may be suspended in its place. It may be constructed at the expence of five or six pounds: that which stands before our window was made for less than three guineas, as we had many of the materials beside us for other purposes.

\* In this experiment, the boy should pull as near as possible to the shaft, within a foot of it, for instance, else he will have such mechanical advantage as cannot be counterbalanced by any weight which the machine would be strong enough to bear.



IV.

*Experiments and Observations on the Light which is spontaneously emitted, with some Degree of Permanency, from various Bodies. By NATHANIEL HULME, M. D. F. R. S. and A. S.*

(Concluded from page 427.)

SECTION IV.

*Other Bodies or Substances have a Power of preserving Spontaneous Light for some Time, when it is applied to them.*

EXPERIMENTS.

**E**XPER. 1. Some luminous matter scraped from the herring, was mixed with a solution of two drams of Epsom salt in two ounces of cold pump water: after shaking very well for some time the phial which contained them, the whole liquid became richly impregnated with light, and continued shining above twenty-four hours. This experiment was frequently repeated, and with the same effect.

*Exper. 2.* Two drams of Glauber's salt and two ounces of water being mixed with herring light, the solution was thereby quickly made very lucent, and remained so until the succeeding evening.

*Exper. 3.* Mackerel-light, being mixed with two drams of Rochelle salt or tartarized natron and two ounces of water, caused the fluid to be very luminous.

*Exper. 4.* Two drams of soda phosphorata and two ounces of water, mixed with herring-light, formed a very lucent fluid, which retained the light for a long time.

*Exper. 5.* Herring-light, with one dram of saltpetre or nitrated kali and two ounces of water, made the solution pretty luminous.

*Exper. 6.* Half a dram of common salt dissolved in two ounces of water, with the addition of mackerel-light, composed a very shining mixture, which retained its splendour for the space of a day or two. The same effect was produced by herring-light.

*Exper. 7.* Two ounces of sea water, being agitated with the light of a mackerel, soon obtained a brilliant illumination. The sea water preserved its luminousness for several days. The experiment was successfully repeated.

*Exper. 8.* Two drams of pure honey, that had not been clarified, or exposed to heat, were dissolved in two ounces of water; and, after the admission of some mackerel-light, and shaking the phial, the solution was fully impregnated with light, which was visible the next evening.

*Exper. 9.* Two drams of purified or refined sugar being dissolved in two ounces of water, and mixed with the shining matter of a herring, the fluid acquired a great degree of lucidness. The same effect took place when the experiment was made with soft brown sugar.

N. B. It is almost needless to mention, that the degree of illumination in these liquids must depend upon the quantity of lucific matter applied; but, in general, as much as can be scraped off by the blunt point of a moderately-sized knife, at a few times, will be sufficient, being assisted by a strong agitation of the containing phial.

#### OBSERVATION.

These experiments enable us to take light and diffuse it through water, so as to render the whole liquid most brilliantly luminous, or, in other words, to impregnate water with light. By these means, the light is so extended in its surface, and combined in such a manner, as to become exceedingly convenient and useful for various other experiments.

#### SECTION V.

*When spontaneous Light is extinguished by some Bodies or Substances, it is not lost, but may be again revived in its former Splendour, and that by the most simple Means.*

#### EXPERIMENTS.

*Exper. 1.* On the 1st of June, 1795, the following experiments were made, to know what was the best proportion of Epsom salt to water, in order to produce the most luminous liquid. Some shining matter was taken from a mackerel, and mixed with a solution of seven drams of the salt in one ounce of water; and its light was immediately extinguished. The same effect ensued, but in a less degree, with a solution of six, and one of five drams. In a solution of two drams, in the same quantity of water, the liquid was luminous; but much more so when only one dram of salt was used. Observing the extinction of light to take place, as above, in the more saturated solutions, while the diluted solutions were luminous, it occurred to me to endeavour to discover what became of the extinguished light, in the former case, and whether it might not be revived by dilution. For this purpose, I took the solution of seven drams of salt in one ounce of water, in which the lucid matter from a mackerel had been extinguished, and diluted it with six ounces of cold pump water; when, to my great astonishment, light in a moment burst out of darkness, and the whole liquid became beautifully luminous! This revived light remained above 48 hours, that is, as long as other light in general does, which has never been extinguished. Hence, it had lost nothing of its vivid luminous powers by its extinction.

*Exper.*



*Exper. 2.* The last experiment was then reversed. A solution of one dram of Epsom salt in one ounce of water, was brilliantly illuminated with mackerel light. Then, six drams of the salt were put into this luminous liquid; and, after shaking the phial very well for a little time, to promote the solution of the salt, the light was totally extinguished. But the same light was again recovered, by the addition of six ounces of water.

In this manner the light may be frequently extinguished, and as often revived. In one instance, the same light, by a repetition of this method, was made to undergo ten extinctions.

*Exper. 3.* A good quantity of herring-light, being mixed with a solution of four drams of common salt in two ounces of water, was immediately extinguished. Then, fourteen ounces of cold pump water were added thereto, and the whole liquid was at once finely illuminated. On the next evening it appeared still very lucid; and likewise on the succeeding night.

*Exper. 4.* The experiment was reversed. Half a dram of the salt, being dissolved in two ounces of water, had herring-light mixed therewith, so as to be made very luminous. On the addition of two drams more of the salt, the lucidness was instantly destroyed: but the light was again recovered, by pouring eight ounces of cold water upon the extinguished luminous fluid. The revived light was very vivid the next evening.

*Exper. 5.* Two ounces of sea water were illuminated with mackerel-light, and then extinguished by adding two drams of common salt. The light was again restored, by diluting the solution with eight ounces of cold spring water.

N. B. If the illuminated liquid be uncommonly brilliant, it may sometimes require more salt to extinguish the light completely, than is here specified; in that case, the measure of water for dilution, must be always calculated in exact proportion to the weight of salt employed.

## SECTION VI.

*Spontaneous Light is rendered more vivid by Motion.*

### EXPERIMENTS.

*Exper. 1.* A quantity of illuminated liquid was poured into a broad vessel, which was placed in the laboratory. The next evening, on examination, it appeared to be quite dark. But a finger, or rod, being drawn through it, was followed by a luminous line.

*Exper. 2.* A phial, containing a pretty large portion of liquid impregnated with light, having been at rest a number of hours, the liquid seemed to have lost its luminous quality, except a little glimmer floating at the top. It was then gently moved, and the light diffused itself gradually through the whole liquid: on agitation, the lucidness was much increased; and, the brisker the motion, the more vivid was the illumination.

## SECTION

## SECTION VII.

*Spontaneous Light is not accompanied with any Degree of sensible Heat, to be discovered by a Thermometer.*

## EXPERIMENTS.

*Exper. 1.* A luminous herring, and another that was quite fresh and not luminous, were placed for a considerable time in the same degree of temperature. A thermometer was then applied to each of them, but no difference of heat could be discovered.

*Exper. 2.* The soft roe of a herring, in an exceedingly lucid state, and a thermometer, were kept together for some time in the laboratory. The roe was then put upon the bulb of the thermometer, without affecting it.

*Exper. 3.* A mackerel, which shone with very brilliant light, was also put to the test of a thermometer, but the instrument remained stationary.

*Exper. 4.* The bulb of a thermometer was surrounded by many small pieces of shining wood, uncommonly luminous, which were kept in that situation for some time; but the light made no alteration upon the thermometer.

*Exper. 5.* Illuminated liquids, and spring water, being kept together in the laboratory, always preserved the same degree of temperature.

## SECTION VIII.

*The Effects of Cold on Spontaneous Light.*

## EXPERIMENTS.

*The Light of Fishes.*

*Exper. 1.* Five small gallipots, containing three pieces of soft-roë of herring, and two of the herring itself, all very luminous, were placed in a frigorific mixture, composed of snow and sea-salt; and, in about an hour and a half, the light was quite extinct, and the bodies totally frozen. The gallipots were then removed into a vessel of cold water, that their contents might be gradually thawed; which being done, they all recovered their pristine luminous state. The pieces were afterwards observed to shine during three succeeding nights.

*Exper. 2.* A small phial, containing three or four drams of liquid impregnated with light, was placed in a frigorific mixture. As the liquid froze, its lucidness gradually diminished; and, when it was quite congealed, the light perfectly disappeared. The phial was then taken out, and put into cold water, at about 49° temperature, that the ice might be gradually liquefied; and, when that was accomplished, the whole fluid became as luminous as before.



*The Light of shining Wood.*

*Exper. 3.* A fragment of shining wood was put into a small wide-mouthed phial, which was plunged into a frigorific mixture. As the cold affected the wood, the light gradually faded, and at last was totally imperceptible. The phial was then taken out, and placed in water at about  $62^{\circ}$ ; by this change of temperature, the frozen wood gradually thawed, and then regained its former lustre.

*The Light of Glow-worms.*

*Exper. 4.* A small phial, containing a luminous dead glow-worm, was exposed to the cold of the frigorific mixture; as the coldness penetrated the phial, the light diminished, and at length was totally extinct. But, by placing the phial in water at about  $62^{\circ}$ , the glowing property of the insect soon returned. In this experiment, the glow-worm was evidently congealed; for it adhered to the side of the glass, and was covered with a hoarfrost. This experiment was frequently repeated, and with the same result.

## OBSERVATION.

By these experiments we learn, that cold extinguishes spontaneous light in a temporary manner, but not durably, as the substances of the third section do; because the light revived again in its full splendour, as soon as it was exposed to a moderate degree of temperature.

## SECTION IX.

*The Effects of Heat on Spontaneous Light.*

## EXPERIMENTS.

*The Light of Fishes.*

*Exper. 1.* One side of a luminous herring was held before the fire, for a short space of time, but so as to receive its heat very strongly. It was then conveyed into the laboratory; when that side which had been exposed to the fire was found quite dark, but the other continued still luminous. The fish was preserved till the next evening, but the extinguished light did not re-appear.

*Exper. 2.* A whole herring, finely shining, was thrown into a quantity of boiling-hot water, and the light was immediately extinguished: after keeping it there for some time, it was taken out, but the light did not revive.

*The Light of Shining Wood.*

*Exper. 3.* A piece of shining wood, its light being very faint, was put into tepid water at about 90 degrees of temperature, and it became in a short time much more lucid. Another piece, at 96°, was rendered beautifully luminous.

*Exper. 4.* A pretty thick piece of shining wood was put into a gallipot, and sunk under water by means of a weight, together with a thermometer, at the temperature of 64°. Boiling-hot water was then added by spoonfuls; and the light, at first, was rendered much more vivid, but soon after began to decrease, and was apparently extinct at about 110°. I say apparently, because on the next evening the light had somewhat revived; which shows, that the heat of 110° was not sufficient to extinguish totally all the light inherent in this piece of wood.

*Exper. 5.* Finding that 110 degrees of heat did not wholly extinguish the light of shining wood, a good many fragments, of different sizes, were then submitted to the power of boiling water, and detained therein for some time, in order that the heat might penetrate them thoroughly. The effect was, that the light became quickly extinct, and did not, as before, re-appear on the following evening.

*The Light of Glow-worms.*

*Exper. 6.* A dead shining glow-worm was put upon two ounces of water, contained in a wide-mouthed phial, at the temperature of 58°. The phial was then sunk, about two or three inches deep, in boiling-hot water; and, as the heat communicated itself to the contents of the phial, the light of the glow-worm became much more vivid.

*Exper. 7.* Another lucid dead glow-worm was put into warm water, at 114°, to see if that degree of heat would extinguish the light; but, on the contrary, its glowing property was augmented. All the water was then poured off, yet the insect continued to shine for some length of time.

*Exper. 8.* The effect of that heat which is obtained from dry, solid bodies by friction, was next tried upon the light of the glow-worm. Two living glow-worms were put into a one-ounce phial, with a glass stopple; and, though they were perfectly dark at the time, yet, if the phial was briskly rubbed with a filken or linen handkerchief, till it became pretty warm, it seldom failed to make them display their light very finely. This experiment was very frequently repeated. It had the same illuminating effect upon the light of a dead glow-worm.

*Exper. 9.* The complete influence of 212 degrees of heat was now applied to the light of a glow-worm, by pouring upon one when dead, but in a luminous state, some boiling water. Its light was instantly extinguished thereby, and did not revive. The experiment was repeated, and with the same result.



*Any of the saline Solutions mentioned in the fourth Section, being impregnated with luminous Matter, and left some Time at rest, are rendered more lucid by a moderate Degree of Heat.*

*Exper. 10.* A quantity of illuminated solution was deposited in the laboratory. The next evening, when it was examined, it appeared in a manner quite dark; but, by putting the phial which contained it into hot water, the light revived, and was soon rendered exceedingly vivid.

*Exper. 11.* About a pint of solution impregnated with light, had become obscure, by time and rest, as is the nature of this mixture. Such a quantity of boiling-hot water was then added to it, as only to give it a small degree of warmth, and it quickly caused it to appear luminous.

*Exper. 12.* Illuminated liquid, to the quantity of four ounces, was placed in the laboratory until the next evening; when it had become almost dark. One spoonful of boiling-hot water being put into it, the light re-appeared; and, by means of two more, it was rendered considerably lucid.

*Their Light is extinguished by a great Degree of Heat.*

*Exper. 13.* Some boiling water being poured upon three or four ounces of illuminated liquid, in an earthen vessel, the light was immediately extinguished; and, though afterwards kept a considerable time for inspection, and often agitated, to stir up the hidden light, yet no remains of any shining property could be perceived. This experiment was frequently repeated, and always with the same result.

*Exper. 14.* Four ounces of very luminous liquid, together with a thermometer, were put into a small earthen vessel, glazed white, the better to reflect light. Boiling-hot water was then added, by spoonfuls at a time, and by slow degrees. The first few spoonfuls made it considerably more lucid; and then, by adding more, the light began to fade, and at length was gradually extinguished. This effect took place, in one instance, when the liquid was heated to  $96^{\circ}$ ; in another, to  $98^{\circ}$ ; and in a third, to  $100^{\circ}$ . Hence, this species of light, when thus united with water, seems to be extinguished at from  $96$  to  $100$  degrees of heat. This is a very elegant and pleasing method, of knowing how much heat is required to extinguish the light; because it measures it exactly, provided the hot water be added in small quantities, and by slow degrees, as above directed. To prevent the possibility of any light reviving after an experiment of this kind, would require a much greater heat than that of  $100^{\circ}$ . The intention of the present experiment was only to show, that all light may be apparently extinguished, at so low a degree of temperature as from  $96^{\circ}$  to  $100^{\circ}$ .

*Exper. 15.* A phial of an ounce and a half was filled with some very luminous liquid, but not corked. It was then suspended by a string, in a quart of boiling-hot water

contained in a white earthen mug, and the light was wholly extinguished in about three or four minutes. After this, the phial was kept in the water some time longer, was then taken out to cool, and well shaken, but the light did not revive. It was examined the next day, and agitated again, but no luminous appearance could be discovered; a proof that all the light had been totally extinguished by the power of heat.

*If much Heat be applied to the Bottom of a Tube filled with illuminated Liquid, which has been some Time at rest, the Light will descend in luminous Streams, from the Top of the Tube to the Bottom, and be gradually extinguished.*

*Exper. 16.* A glass cylindrical tube, closed at one end, being 9 inches long, with a bore of  $1\frac{1}{16}$  inch, when used, was put into a gallipot  $3\frac{1}{2}$  inches deep, and  $3\frac{1}{2}$  wide, which held about 12 ounces of boiling water, and was placed in another larger vessel, to receive the overflowing water upon the immersion of the tube. The tube being filled over night with some very luminous liquid, was placed in the laboratory until the next evening. The light had then ascended plentifully to the top of the fluid, (the rest being dark,) and, taking the circular shape of the tube, formed a very lucid ring. The vessels with the boiling-hot water were then carried into the dark laboratory; and the tube being gently and carefully placed (without shaking) in the gallipot, the light was, generally in about half a minute, seen plainly to descend in streams from the top to the bottom; illuminating the whole fluid in its descent in a beautiful manner, and then was gradually extinguished. The extinction of the light began at the top of the tube, and ended at the bottom.

*Exper. 17.* The experiment was also made with a tube 19 inches high,  $\frac{1}{2}$  an inch in bore, having several curvatures, and sealed hermetically at its lower end. Both extremities were made straight for a few inches; the one to be immersed in the water, and the other to prevent the liquid running out. The luminous ring being formed as above mentioned, the tube was put into the gallipot of boiling-hot water; and, in a short time, the light began to descend from the top, and came waving down, in a pleasing manner, to the bottom of the tube in the hot water, and then was by degrees extinguished. The whole length of the tube, including the curvatures, was 26 inches.

The most eligible solutions for this curious experiment, are those made with Epsom salt, Glauber's salt, sea-salt, and sal ammoniac: if either of the two former be used, the proper proportion is, one dram of salt to each ounce of water; if either of the two latter, 15 grains to each ounce of water will be sufficient.

N. B. The experimentalist, before he views the descent of the light in the tube, should always remain in the dark for some little time, in order to get rid of all extraneous light adhering to the organs of vision, and to accommodate the eye to darkness.



## SECTION X.

*The Effects of the human Body, and of the animal Fluids, upon spontaneous Light.**The living Body.*

*Exper. 1.* On touching the luminous matter of fishes, the light adhered to the fingers and different parts of the hands; remained very lucid for some little time, and then gradually disappeared. But the same kind of matter being applied to pieces of wood, stone, and the like, of the same temperature as the laboratory, continued luminous on these substances for many hours.

*Exper. 2.* A piece of red blotting-paper, about one inch square, and four times doubled, was finely illuminated by matter from a herring, and applied to the upper part of the inside of the thigh. After the expiration of 15 or 20 minutes, it was taken off; and, on examination, the light was quite extinguished. The experiment was repeated several times, and with the same effect. Another piece of the like paper was illuminated at the same time, and placed in the laboratory; where it retained its light above 48 hours.

*Exper. 3.* A piece of shining wood was placed upon the palm of the hand, and inclosed therein for some time; on inspection, it was found to be more lucid than before. Many trials of this kind were made, with the like success.

*Exper. 4.* A dead glow-worm, being but slightly luminous, was breathed upon several times; and its light increased both in magnitude and brightness. The experiment was frequently repeated, with the same result.

*Animal Fluids.**Blood.*

*Exper. 5.* A person having received a contusion, but otherwise in health, was bled. The next day, some herring-light was mixed with about two ounces of the crassamentum or red coagulated part of the blood, by stirring them well together with a knife: it caused it to be slightly luminous, but the light was not of long duration. Nearly the same result followed the mixture of lucid matter with the recent crassamentum of persons labouring under inflammatory diseases, as the pleurisy and rheumatism.

*Exper. 6.* But, when mixed with crassamentum that had been kept for some time, and become black and somewhat offensive to the smell, the light seemed to be more quickly extinguished.

*Exper. 7.* A singular phenomenon happened several times, on mixing fish-light with putrescent bloody serum. It would not incorporate, but was ejected in globules, like quicksilver when rubbed with any unctuous substance, and afterwards adhered to the side of the vessel in which the mixture was made, in the form of a lucid ring.

*Exper. 8.* The luminous matter of a herring was mixed with about two ounces of pure serum, from the healthy subject of the 5th experiment: it soon became finely illuminated, and retained its shining appearance for a long time, whenever it was stirred or agitated.

*Exper. 9.* The recent serum, drawn from patients afflicted with inflammatory complaints, was illuminated pretty much in the same manner as in the 8th experiment; and often retained light above 48 hours.

#### *Urine.*

*Exper. 10.* Mackerel-light being mixed, by strong agitation, with some fresh urine from a healthy person, a glimpse of light was retained at first, and then was gradually extinguished. But stale and pungent urine, being incorporated with luminous matter, had still greater extinguishing effect.

#### *Bile.*

*Exper. 11.* Some bile, taken from a person who died of a suppression of urine, had herring-light mixed with it, which soon became extinct. Another trial was made, with a different bile, and with the same result.

#### *Milk.*

*Exper. 12.* Human milk not being easily obtained, some mackerel-light was incorporated, by agitation, with two ounces of fresh cow's milk, which was thereby rendered finely luminous, and continued shining above 24 hours. Fresh cream also retained some light; though it was not so visible as with milk, owing probably to its thickness. But, when either milk or cream turn sour, they contract a very extinguishing property. A quart of milk was kept five days, in a moderately cool place, in the month of June; by that time, it was changed into a mixture somewhat resembling curds and whey, that is, into a small smooth coagulated part, and a very thin one, both which were acidulous. Some fine mackerel-light was mixed with two ounces of each of them, in separate phials, and they extinguished it immediately.

### *V. Observations*



V.

*Observations on the Theory of Electric Attraction and Repulsion. By the Rev. GEORGE MILLER, F. T. C. D.\**

**B**EFORE that the theory of a single electric fluid was proposed, no difficulty occurred in the explanation of the attractions and repulsions observed to arise from electricity. If we admit that there are two distinct electric fluids, each of which strongly attracts the other, but consists of particles mutually repulsive; it becomes easy to account for the attraction subsisting between bodies in different states of electricity, and the repulsion between those in the same. But when Dr. Franklin †, observing that a man, standing upon a non-conductor, could not electrify himself, but that he could electrify another person also standing upon a non-conductor, was induced to regard the operation of exciting electricity only as a transfer of one and the same fluid from one body to another; it was found to be difficult to reconcile to the new theory the mutual repulsion of bodies in that state which is, according to this theory, denominated negative electricity. Dr. Franklin ‡ acknowledged that he could not assign a satisfactory reason for it; and Dr. Priestley § has proposed it, as one of the queries remaining to be solved for completing the science of electricity. Many attempts have been made to obviate this apparent objection to the simple theory of a single fluid; but the difficulty seems still to be as great as it was in the time of Franklin.

|| Æpinus has applied a very elaborate system of mathematical reasoning to the solution of electrical phenomena, and has adopted as the basis of his theory, the same opinion which Franklin had entertained concerning the nature of the electric fluid; but he has combined with this opinion other principles so inadmissible, that his reasonings cannot be regarded as just explications of the phenomena. He has assumed, apparently without any other reason than its importance to his conclusion, that the particles of all other substances repel each other. His system must therefore be considered, not as a physical solution agreeable to the known laws of natural operations, but merely as an ingenious exercise of mathematical ability.

M. De Luc, who rejected the solutions of Æpinus has endeavoured to supply the deficiency. \*\* Having remarked that the divergence of the balls of an electrometer, included in the receiver of an air-pump, is continually diminished during the progress of exhaustion; he considers it as proved, that the cause of all electrical movements, whether of attraction or of repulsion, is the action of the air. This principle he applies in the following manner. When two bodies are in similar states of electricity, either positive or negative, they will

\* Irish Transactions, VII. 139.

† Dr. Priestley's History of Electricity, p. 161.

‡ Dr. Priestley's History of Electricity, p. 165.

§ Ibid. p. 492.

|| Journal de Physique, Dec. 1787.

\*\* Ibid. Juin 1790.

conspire to modify, either by giving or receiving the electric fluid, the state of the intermediate air, whilst that of the exterior air is only modified by either of them singly; and therefore the state of the exterior air will differ more from that of the electrified bodies, than the state of the intermediate air. In this case he contends that a repulsion must take place, because each body must move towards that part of the surrounding medium, whose electrical state is most different from its own. On the other hand, when bodies are in different states of electricity, they will mutually counteract the changes, which they might separately produce in the state of the intermediate air; but each will operate on the exterior air without any compensation. In this case the state of the intermediate air will continue to differ from that of each body as much as at the first instant, whilst the state of the exterior air is separately modified by each body according to its respective state of electricity. The two bodies therefore, moving towards that part of the surrounding medium, whose electrical state is most different from their own, will at the same time move towards each other.

This theory very ingeniously avoids the difficulty of explaining the case of electrical repulsion, by resolving it into an attraction towards the surrounding medium. It seems however to be liable to two objections. In the first place, instead of assuming unauthorized principles with the preceding theory, it omits the consideration of one whose existence seems to be ascertained by experiments. If a body be in either state of electricity, it will induce in an adjacent body the contrary state, until it shall have come within a certain distance. This property, which has been ascertained by various experiments, indicates a repulsive force subsisting between the portions of the electric fluid that belong to the adjacent bodies; and this theory makes no allowance for such a repulsion. The fundamental principle of it is merely a diffusion of the electric fluid, and is \* thus stated by M. de Luc: "the electric matter tends towards all substances, and the more strongly in the same proportion in which they possess a smaller quantity." In the second place, it does not appear, when carefully considered, to afford any assistance towards the removal of the grand difficulty, the mutual repulsion of bodies negatively electrified. If two bodies negatively electrified be placed at a small distance, they will both, according to M. de Luc's explanation, receive the electric fluid from the intermediate air, which will consequently retain a smaller portion than the surrounding atmosphere. From the law above-mentioned it should follow, that the redundant fluid of the exterior air should by diffusion be communicated both to the bodies and to the intermediate space; but no reason appears, which would induce us to suppose that the bodies themselves should recede to a greater distance. M. de Luc does indeed endeavour to prove that such a motion should take place, but by an experiment whose solution contradicts his own theory. He suspended by a silk thread a large, but light, metallic ball, and presented it in a state of positive electricity to a body

\* "La loi suivante suffit seule: La matière électrique tend vers toutes les substances, d'autant plus fortement, qu'elles en possèdent moins." *Journal de Physique*, Juin 1790.



negatively electrified. The former was attracted towards the latter until it arrived at a certain distance, at which it discharged its electricity. Hence he concluded, in general, that when a body has more of the electric fluid than the neighbouring bodies, and is less disposed to resist its own motion than to abandon the excess of its electric matter, it will move towards that place which contains less of this matter. But in this experiment he considers the two bodies as acting on each other at a distance without any reference to the intermediate air.

Mr. Cavallo\*, in the last edition of his treatise on electricity, has observed, that the mutual repulsion of two bodies negatively electrified is still supposed to contradict the theory of Franklin; and has therefore deemed it necessary to obviate the objection by a very particular detail. For this purpose he has premised the following propositions: Prop. 1. No electricity can appear on the surface of a body, or no body can be electrified either positively or negatively, unless the contrary electricity can take place on other bodies contiguous to it. Prop. 2. There is something on the surface of bodies, which prevents the sudden incorporation of the two electricities, viz. of that possessed by the electrified body with the contrary electricity possessed by the contiguous air, or other surrounding bodies. Prop. 3. Supposing that every particle of a fluid has an attraction towards every particle of a solid; if the solid be left at liberty in a certain quantity of that fluid, it will be attracted towards the common centre of attraction of all the particles of the fluid. To this last proposition he has subjoined the two following corollaries: 1. † the same thing must happen, when the quantity of fluid is smaller than the bulk of the body; 2. if the attraction of the particles of the fluid be exerted only towards the surface of the solid, the effect will be the same when the body is of a regular shape; but the difference will in any case be inconsiderable.

With regard to the solution founded upon these principles it must be remarked, that it is not derived simply from a consideration of the supposed nature of the electric fluid; but from a mixed statement of that nature and of properties assumed merely from experiments as matters of fact. The first and second propositions express those properties, and, though the experiments to which the former refers, may be explained by ascribing the phenomena to the repulsive nature of the fluid, yet the latter is assumed without any such reference. "Without examining," says Mr. Cavallo, "the nature, the extent, and the laws of this property in bodies, it will be sufficient for the present purpose to observe, that the fact is certainly so; for otherwise a body could not possibly be electrified, or it would not remain electrified for a single moment." From these principles thus assumed, Mr. Cavallo deduces the existence of atmospheres of contrary electricity existing in the air contiguous to the bodies; and from the attractions which are thereby occasioned he infers the apparent repulsion of the electrified bodies.

\* Vol. III. p. 192.

† Of this corollary Mr. Cavallo does not appear to make any distinct application.

If these atmospheres be conceived to be formed by the repulsive nature of the fluid, some allowance should be made for the mutual repulsion of the two redundant portions belonging to bodies positively electrified. This however seems to be neglected for the purpose of explaining the repulsion of bodies negatively electrified. But the difficulty seems to be only changed. If the negative atmosphere adjacent to a body positively electrified be caused by the repulsion of the redundant fluid of the body, it will be necessary to shew that this repulsion is overpowered by the attraction subsisting between that redundant fluid and the portion of air thus deprived of a part of its electric fluid.

But the reality of these atmospheres of contrary electricity may well be questioned. It seems to require, that we should conceive a portion of air contiguous to each body to be permanently, during the mutual repulsion of the bodies in a state of electricity opposite to that of the bodies. But \* it is ascertained experimentally, that the air surrounding any electrified body acquires the same electricity which had been possessed by the body, and retains it even after the removal of the body. This must be supposed, agreeably to the known laws of electricity, to be communicated by the alternate attraction and repulsion of the adjacent particles of air. Each particle must be first attracted towards the body, and, when by contact it has acquired the electricity of the body, repelled from it. Instead therefore of a permanent state of contrary electricity constituting these supposed atmospheres, each adjacent space must be occupied by particles, some of which are attracted and others repelled. The time requisite for thus reducing the electricity of the body to an equilibrium with that of the surrounding air, is sufficient for explaining the continuance of the electricity of the bodies, without the aid of the second proposition; and the first proposition is deduced only from a consideration of bodies in a solid state.

Possibly a more distinct application of a principle, already in some degree adopted both by Doctor Priestley and Mr. Cavallo, may remove all the difficulties of this inquiry. At least I will hope, that it may lead to such a consideration of the question, as may subject the merits of the theory itself to a fair and decisive discussion. This principle is saturation. † Doctor Priestley has explained the communication of the redundant fluid of a body positively electrified to another, a part of whose fluid had been previously expelled, by supposing that it was more strongly attracted by the other body, than by its own which had more than its natural share; and ‡ Mr. Cavallo has in the same manner accounted for the mutual attraction of bodies in different states of electricity.

In applying this principle to the solution of electric phenomena three forces must be considered: 1st. the attraction subsisting between each body and its own portion of the electric fluid; 2dly, the attraction which may subsist between each body and the portion of fluid belonging to the other; and 3dly, the repulsion subsisting between the two portions of the electric fluid.

\* Cavallo's Complete Treatise on Electricity, Vol. I. p. 326.

† History of Electricity, p. 253.

‡ Vol. I. p. 109.



That the attraction subsisting between two bodies in opposite states of electricity may be explained, it is necessary to consider previously the case of two bodies in their natural or ordinary state. In this case the force subsisting between each body and its own portion of the electric fluid is not in a state of saturation, because it must be sufficiently strong to counterbalance the elasticity of the fluid. Each body is therefore still capable of being attracted by the fluid belonging to the other, and each portion of the fluid is also capable of such attraction. This force, if it should operate alone, would draw the bodies together; but the mutual repulsion of the two portions of the fluid tends to produce the opposite effect. The quiescence of the bodies proves the equality of these forces.

If two bodies in opposite states of electricity be brought together, the body positively electrified cannot be attracted towards the remaining electric fluid belonging to the other, because this body may be considered as saturated with the fluid, and that portion of the fluid as saturated with solid matter. For the opposite reasons an attraction will take place between the body negatively electrified and the fluid belonging to the former. It remains to be shewn, that this attractive force may exceed the mutual repulsion of the two portions of fluid. It must be observed, that the repulsion remains the same, because the sum of the two quantities of fluid is not altered; whereas the attraction is augmented by the unequal distribution of the fluid. The one body is charged with more fluid than that which its own attracting force is capable of retaining, and the redundant fluid will consequently be strongly impelled towards the other body, whose attractive power is at the same time increased by the deficiency of its own portion of fluid.

In the case of two bodies similarly electrified the bodies may be either both positively, or both negatively electrified. When they are both positively electrified, they are both saturated with the electric fluid; and when they are both negatively electrified, both remaining portions of the electric fluid are reciprocally saturated with solid matter. In neither case therefore can any attraction take place between either body and the fluid belonging to the other. Consequently, the repulsion existing between the two portions of the fluid must operate without resistance, and the two bodies be repelled from each other.

Should this solution of electric attraction and repulsion be admitted, it will perhaps also remove the difficulty of magnetic repulsion. In this part of philosophy it has been found difficult to explain the repulsion of the corresponding poles agreeably to the theory of a magnetic fluid. In every magnetical body the equilibrium of this fluid is supposed to be disturbed, and one part of the body is conceived to be overcharged with the fluid, whilst the other is undercharged. The difficulty was to explain the repulsion of the undercharged poles, as in electricity to explain the repulsion of bodies negatively electrified. Mr. Kirwan has indeed, in a Memoir contained in the Sixth Volume of the Transactions of the Academy, referred the phenomena of magnetism to crystallization; but his mention of the term *saturated* in that Memoir seems to imply, that he does not mean to exclude the supposition of a magnetic fluid. If this be adopted, the preceding solution may be applied to the phenomena of magnetism, in the same manner in which it has been already applied to those of electricity.

The theory, according to which the preceding solution has been proposed, supposes the electric fluid a *single* fluid; but it is not necessary that it should be conceived to be absolutely *simple*. We know, for instance, that atmospheric air is a combination of at least two distinct fluids; and yet explain the phenomena of the barometer, air-pump, and condenser, as depending merely on its presence or absence, without any reference to the composition of its nature. In the same manner some electric phenomena may be justly explained by considering them as the effects of the different distribution of the same fluid; whilst its phosphoric smell, its power of changing blue vegetable colours to red, and its combustion may possibly be derived from its decomposition.

## VI.

*Description of a new Rotatory Engine for raising Water, and for other Purposes: By a Correspondent.*

To Mr. NICHOLSON.

SIR,

I SUBMIT to your judgment, and shall be much gratified by your opinion on the engine of which I send you the inclosed drawing. If you think it may be intitled to a place in your collection, either as an apparatus, or as affording some hints to the improvement of hydraulics, the whole of my views respecting it will be answered. I will confess to you, that I have not constructed it upon a scale of actual work, and am well aware how many subordinate points of organization require to be settled, before any mechanical contrivance can obtain its best and most effective form: but as it has already been the subject of a considerable degree of meditation, I have ventured to offer it to you in its present state.

I am, Sir,

Your obliged Servant,

London, Dec. 14, 1800.

O. B.

## DESCRIPTION.

In Plate XX. the upper figure represents an horizontal section of the main part of the engine, and the lower figure shews the same engine, as it would appear to an observer viewing it at right angles to the former direction of sight: the same letters in both denoting the same parts. A A represents an elliptical vessel of wood, or rather of metal, having its top and bottom flat, and joined to the sides by flanches or borders. The sides are upright, and may be of any height according to the intended capacity. In this vessel revolves the cylinder B, B, having the same axis, and touching the internal circumference at two opposite



opposite lines, where the place of contact is made water tight by leathering, packing, or any of the usual methods. At opposite parts of the surface of the cylinder are two flaps C, C, which are urged outwards by springs, which cause them to bear against the interior surface of the ellipsis, and when pressed down they fall into cavities in the cylinder, so as to complete the defective portion of its face. It must be understood that these flaps, as well as the cylinder itself, are of the height to fill up the interval between the flat faces of the top and bottom, and are secured so as to be water tight. Lastly, E E are two pipes leading downwards to the water intended to be raised, and F F are two pipes leading upwards to the reservoir, or receptacle, to which it is to be conveyed. The first mover is applied at the extremity D of the axis.

The action. Let us suppose an engine of this construction to be fixed on board a ship centrically beneath one of the capstans, and that by the common contrivance of a gear bar, or other equivalent piece, the lower extremity of the axis of the capstan is connected with that of the machine, and a convenient number of men set to work it in the direction H G. The immediate consequence will be, that the spaces H, H, behind the flaps C, C, will be enlarged, and as they are air tight, the pressure of the atmosphere will drive water from the well through the pipes E E into those spaces. As the flaps C proceed towards F, they will be pressed in by the elliptical concavity, and at last the spaces G G will disappear, and the greatest part of the spaces H H (then constituting the whole internal part of the vessel) will be nearly full of water: By the continuance of the process the two flaps will have passed the places of contact, and begin to open on the opposite sides of the ellipsis, driving before them the water and air up the pipes F F; at the same time that the spaces H H become again enlarged, and draw a fresh supply of water through E, E. A constant and almost equable stream is thus produced through the apparatus, which has this advantage, that by increasing the number of men at the capstan, the velocity of this current may be increased, so as to equal, as it should seem, any exigency the state of the vessel might produce.

If the suction pipes were occasionally led into the sea, and the forcing pipes into a moderate sized air vessel, like that of the fire engine, a very powerful stream of water might be directed to any part of the ship, in case of the dreadful accident of fire; or for the common purposes of cleanliness of the births of men or animals, upon which so much of their health and comforts depend.

Shall I extend my speculations farther, and express my conjecture, that this might form no contemptible steam engine, if steam were admitted through E into H, and condensed so as to render the pressure of the atmosphere active in G upon the flap, when the condensation were made; or that it might receive the action of a descending stream of water, and work a mill, or other useful machine, &c. &c. Or shall I not rather leave this visionary region of fanciful indulgence, and commit the fate of my engine at once to the censure or praise of yourself and readers?

O. B.

\*\*\* Many

\* \* Many engineers have exercised their talents in contriving machines which should raise water without the alternate action of the pump; but I do not recollect having seen any construction precisely the same as that of my correspondent. In the *Recueil des Machines et Inventions approuvées par l'Académie Royale des Sciences*, tom. I. page 103, there is a machine by the celebrated M. Amontons, for raising water by a drum or cylinder, fixed with its axis horizontal, and an ellipsis revolving in it against two flaps or valves attached to the cylinder; the effect of which was to afford two pair of variable spaces similar to the engine before us. It is dated 1699.

Mr. Thomas Dickinson in 1790 obtained a patent for a new engine on a rotatory principle, of which the contrivance shews a mind habituated to mechanical research and invention. The principal organ consists of a cylinder, in which another eccentric cylinder revolves, leaving a gibbous space on one side, and out of this last cylinder issues two sliders crossing each other, and so contrived as to length and other expedients, that they sweep the cavity, and afford variable spaces for the introduction and extrusion of water. The drawings may be seen in the second Volume of the Repertory of Arts, and the whole differs very much from the invention of my correspondent.

The action of steam against flaps or valves between two concentric cylinders, forms part of some imperfect descriptions in the specifications of the celebrated James Watt, and there is also a contrivance of this kind loosely described in the first volume of the Irish Transactions, by John Cooke, Esq.

I shall not attempt to discuss the relative values of these inventions, either with regard to each other, or to the engines more generally used. The advantage of an incessant or continued motion is admitted by all mechanics, and might be easily displayed. But in engines like every one of the foregoing, the difficulties of stuffing or packing the parts which move in contact, and the considerable friction and wear they must be subject to, unless improved far beyond their present state, are so great, that I think few practical men would venture to undertake them. I do by no means presume to say, that they are on that account useless. Undoubtedly they may be classed at least with those products of the understanding, by which our knowledge and mental habits are improved, and which form a stock whence the most striking and unforeseen discoveries are occasionally drawn. Sheep were marked, loadstones were examined, and the covers of kettles were blown off many ages before the art of printing, the mariner's compass, and the steam engine were thought of.

W. N.



## VI.

*Account of a new Method of bleaching Cotton, as published by Chaptal, Member of the National Institute. By J. C. DELAMETHERIE \*.*

THE successful experiments made by Berthollet in bleaching vegetable cloth, by means of the oxygenated muriatic acid, seem to have brought this art nearly to a state of perfection; but this method is not in every instance equally economical: it requires to be performed by skilful operators, in order that the goods may not be affected by a ley too corrosive, or applied at an improper time, independent of which consideration, it is desirable that every process should be completely disclosed, in order that the artist may choose such means as may be best suited to his pursuits.

This consideration has induced me to describe a very simple and economical process for bleaching cotton thread; it is as follows:

At the height of about four decimetres (18 inches) and an half above the grate of a common furnace, a copper boiler is placed, of a round form, five decimetres (20 inches) in depth, and one metre and a third, ( $3\frac{1}{2}$  feet) in diameter. The projecting rim of the cauldron, which is about two decimetres (8 inches) rests upon the brick work of the furnace. The remainder of the kiln is raised of free stone, and forms an oval boiler or digester about two metres (6 feet and a half) in height, and its width, when measured at the centre, is about one metre and a third, or  $3\frac{1}{2}$  feet.

The upper part of this vessel has a round orifice about half a metre ( $19\frac{1}{2}$  inches) in diameter, which is closed, when necessary, by a large moveable stone, or by a copper lid, adapted for the purpose. On the flanch of the copper vessel, which forms the bottom of this kind of digester, a grating is laid, which consists of bars of wood, placed near enough to prevent the cotton that is put on them from falling through, and sufficiently strong to support the weight of eight hundred kilogrammes, (or  $1\frac{1}{2}$  cwt.)

When this structure is completed, the cotton having been previously divided into hanks or parcels is slightly impregnated with a solution of soda, rendered caustic by the addition of lime. This operation is performed in a trough of wood or stone, in which the cotton is worked by men with their feet defended with wooden shoes. As soon as the cotton is sufficiently impregnated with the alkaline liquor, it is conveyed to the digester, and piled upon the wooden grate. In this situation the exuding liquor runs through the bars into the copper boiler, where it forms a stratum of fluid, and which allows the whole mass to be heated without danger of burning either the cotton or the metal. The alkaline ley is composed of Alicant soda, in quantity one tenth part of the weight of the cotton, and in a vessel of the dimensions above described, there may be employed at each time, forty myria-

\* Journal de Physique, Vendemaire, an. 9, page 305.

grammes (about 800 French pounds, or 880 avoird.) of cotton. The density of the ley is in general about two degrees, (qu. by what instrument?) After the cotton is properly disposed in the boiler, the cover is put on, and scarcely any issue left for the vapours disengaged by the heat, in order that they may acquire a more considerable degree of heat, and re-act with increased force upon the cotton. When the digester is charged, the fire is lighted in the furnace\*, and the ley submitted to a gentle ebullition from twenty to thirty-six hours. It is then left to cool, the cover is taken off, the cotton carefully washed, and exposed in the bleaching ground for two or three days, by spreading it on frames during the day, and spreading it on the grafs at night. Thus the cotton acquires a beautiful degree of whiteness; and if by accident some portions of the skains should remain unbleached, they are replaced in the boiler, and subjected to a second operation, or otherwise they are left in the field for several days longer. These shades in the bleached cotton are the consequence of its not having been completely and equally impregnated with ley; but they may also proceed from too close pressure in certain parts, at the time of stowing it in the boiler. When the ley is supposed to be exhausted by ebullition, the boiler is uncovered, and the cotton wetted with an additional quantity of the solution of soda; for unless this precaution is taken it may easily burn.

From these particulars it would be easy to form a notion of the œconomy of this process, by calculating the price of the articles, and the time employed in the operation, if there were not a more simple method of estimating its advantages, namely, the low price at which cotton is bleached in all the manufactories where this discovery is practised. In the south of France, where it is now generally adopted, cotton is bleached at the rate of about eight francs, for forty kilogrammes, (or rather cheaper than a penny a pound.) This process was brought from the Levant, a short time after we had obtained the method of producing the Adrianople red. It was reduced to practice, but hitherto kept secret, under the name of *blanchiment à la fumée*, (vapour bleaching.)

*Extract from the Bulletin of the Philomatic Society.*

This process has been employed with the greatest success by Bourlier, at Bons-Hommes, near Paris.

The English, who never neglect an opportunity of improving their manufactures, made trial of the process of Chaptal, as soon as they received information of it. It has completely succeeded, as appears by the following extract of a letter from a man of science at London†.

“A new method of bleaching has just been tried at Balynah, and has completely succeeded. The principle of the process appears to have been published by a French

\* The construction here described is for a fire of pitcoal; but if wood be used, the dimensions of the fire-place must be different. In the latter case the grate would be useless, and the bottom of the vessel too far from the fire.---Delametherie.

† The extract is in French, from which I retranslate.---N.

“ chemist,



“ chemist, Chaptal, who is much respected by our manufacturers. I speak of the art of  
“ discolouring piece goods in a digester, by caustic alkaline ley. Though our first at-  
“ tempts did not perfectly succeed, we were not discouraged. The linen was exposed  
“ to the action of vapour in the apparatus, but it was not equally affected, as it appeared  
“ to be blotched in several places; we were, therefore, obliged to construct an apparatus,  
“ in order to unroll and separate the goods, and to expose the greatest surface possible to  
“ the action of the vapour. Suppose the boiler of a steam engine, in the form of an  
“ elongated ellipsis, provided with a safety valve, two tubes with cocks, to shew the con-  
“ sumption of the liquor, and a mercurial gage, to ascertain the strength of the steam.  
“ This boiler is bedded in masonry, or brick work, that it may resist the excessive pressure  
“ which necessarily takes place. In the interior part of the apparatus are six reels, three  
“ at each end, alternating with each other, in order that the action of the steam may be  
“ more equable upon the goods. These reels are slowly and uniformly carried round  
“ by simple tooth and pinion work of wood, and the first motion is given to an axis  
“ which passes out of the boiler through a stuffing box, which prevents the escape  
“ of vapour. At the top is an opening of about sixteen inches diameter, with  
“ a rim or flanch, on which the cover is fitted, and firmly secured by screws. Between  
“ the two metallic faces are placed strips of soaked leather, to prevent the vapour from  
“ escaping. When the cover is taken off, the workmen can enter the boiler, to dispose  
“ the goods upon the rollers, each of which contains about fifteen or twenty pieces,  
“ making in the whole about forty-five or sixty. The raw material, namely, cunnamara  
“ kelp, is an article of inconsiderable expence, or else the soda extracted from sea salt,  
“ in which there remains indeed a small portion indecomposed, but which we procure at a  
“ very reasonable price. It is rendered caustic by the addition of some good lime, which  
“ is made from our lime-stone of Parre, with these a ley is formed, which is equal to  
“ fourteen degrees of our hydrometer. In this lixivium the piece goods are boiled, and  
“ then conveyed to the digester, on the bottom of which the ley stands to about five inches  
“ in depth. The workman stands upon a perforated stage, which prevents him from  
“ stepping into the ley while he is arranging the pieces: after which, having placed them  
“ on the rollers, the apparatus is closed, the fire lighted, and the operation begins. As  
“ soon as ebullition takes place, the handle on the outside is incessantly turned, and as  
“ soon as the roller at one end is filled, the handle is shifted to the other roller, and the  
“ turning performed in the contrary direction. In this manner the operation is continued  
“ till the whole of the contents is bleached. From this description you may easily understand  
“ how this operation is performed; I shall, however, take the first opportunity of sending  
“ you a plan and description of the apparatus, if you wish for further information. You  
“ are at liberty to make whatever use you please of this account: the expence of bleaching  
“ is not more than one farthing per yard, including coals, workmen’s wages, &c. as well  
“ as interest for the capital employed in the apparatus.”

## VII.

*On the Chemical Effects of the Pile of Volta. By a Correspondent.*

TO MR. NICHOLSON.

S I R,

**T**HERE is something so fascinating in the ease with which the present system of chemistry can be applied to most of the phenomena of nature and art, that I may suffer the derision perhaps of many philosophers, if I even question its application in any. However, as truth should be the object of all who cultivate a knowledge of nature, I shall venture to mention a few circumstances.

I have lately repeated most of the experiments which have been made on the pile of Volta, and have read with much attention the opinions which you have published of your own, and of some of your learned correspondents.

When two wires of platina are used, and when they are placed in water, two gases are produced, the one having the properties of oxygen, the other of hydrogen gas. These wires may be placed at very considerable distances from each other, and yet if they are in the same vessel of water, they produce these airs as speedily and in as great quantities as when they are ever so close to each other.

Now, Sir, I wish to know how it happens, according to any system, that the two component parts of water should be made to appear at such distances from each other. Does the hydrogen of the decomposed particle of water on the zinc side of the pile, fly away instantly as the oxygen is produced on that side, to the wire connected with the silver? If it does, why do we not see the bubbles in its passage? Or does the oxygen pass from the wire connected with the silver to that connected with the zinc? Or are there two currents.

In the ordinary modes of reasoning on these subjects, we generally suppose that when one of the component parts of a substance is separated or is fixed, the other appears instantly in some way or other, and close to it. If, for instance, a bar of red-hot iron be immersed in water, the oxygen unites with the iron, and forms an oxide, and the hydrogen arises immediately from it, wherever the oxygen becomes fixed. This seems perfectly well explained by the present theory of chemistry. In the pile, if copper wires be substituted for platina, one wire will be oxidated whilst the hydrogen arises from the other at a distance. When the phosphuret of lime is dropped into water, the bubbles of phosphorated hydrogen appear close to it, as soon as the oxygen unites with the said phosphorus. This also appears to be well explained by the modern theory. If the particles of water be composed of the two bases of oxygen and hydrogen gas, and if one of those particles be decomposed, and one of its principles appear, I contend that some



account should be given of the other. It is a new principle for it insensibly to hurry through the water for a distance of six inches or more, and there to make its appearance in the character of gas. Volta's discovery of the pile seems to open a very large field of inquiry; it has already altered the arrangement of many facts in philosophical chemistry. As an individual experimentalist, I can say it has led me to many investigations which have explained difficulties which heretofore considerably perplexed me.

I may hereafter take the liberty of submitting to you some further experiments and observations on this subject, and some opinions respecting the generally received doctrine of the decomposition of water.

If you should deem this worthy a place in your learned Journal, by inserting it you will oblige

Your obedient humble servant,

AN EXPERIMENTALIST.

December 21, 1800.

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\* \* The elucidation of the above and many other difficulties of the new galvanic philosophy must be left to the experimental researches of those able men who are now employed upon it. But I may here remark that it is probable that the local proximity of chemical effects dependant on each other may not be such as my correspondent apprehends, but that some distance both of space and time may intervene between all such phenomena. The current from the pile may perhaps teach us to generalize and correct our notions on this head. Is it not a parallel fact in chemistry that the vegetations of a metal (precipitated from an acid by the contemporaneous solution of another metal) are formed and deposited at the distance of many inches from the place of solution? When lead, for example, is precipitated by zinc, and the vegetation towards the end of the process is formed near the bottom of a tall vessel,—is not the solution of the zinc an evidence, according to our common process of reasoning, that lead exists also in the solvent at that place of action; and if so, why is not that lead separated instead of the other very remote portion?—N.

## VIII.

*Memoir of several newly discovered Properties of phosphorated Hydrogen Gas. By Citizen RAYMOND, Professor of Chemistry at the Central School of the Department of the Ardèche\*.*

WE are indebted to Citizen Gengembre for the important discovery of phosphorated hydrogen gas. Before his time no chemist had discovered an elastic fluid, which possessed the remarkable property of taking fire by the contact of air alone, without the necessity of increasing its temperature, or placing it in contact with a body in a state of ignition.

The undulated, and continually increasing crown, which is afforded by this gas when burned by a single bubble at a time, in any place where the air is perfectly calm; the lustre and magnificence which attend this combustion, when effected in pure oxygen gas; the sudden penetration of the two gases, and their total conversion into water and phosphoric acid. These were the only known facts which had interested chemists in the history of phosphorated hydrogen gas when I attempted to discover whether this elastic fluid did not possess other properties, which, although they might not afford a spectacle of equal brilliancy with those above-mentioned, might not on that account be less worthy the attention of philosophers.

The well known property imparted by sulphur to hydrogen, and reciprocally by hydrogen, namely, that both, when combined, are soluble in water, though each, taken separately, is perfectly indissoluble, had already led me to imagine that the same effect might take place in a combination of phosphorus and hydrogen; and that these substances being previously united, might then perhaps become susceptible of partaking of the liquidity of water, by communicating to the latter some new properties, nearly similar to those possessed by the solution of sulphurated hydrogen gas, commonly called hepatized water.

To destroy or effectually to confirm this suspicion, I took a flint glass decanter, which I filled with newly distilled water, of the temperature of twelve degrees; I then inverted it on the shelf of a pneumatic trough, in order to receive the phosphorated hydrogen gas, which is afforded by the decomposition of water, by a mixture of phosphorus and recently slaked lime. (This process is described in the *Annales de Chimie*, for the year 1787.) As soon as the decanter was about half full of phosphorated hydrogen gas, I removed it from the shelf, carefully closing its orifice with my finger, and then briskly shaking it, in order to cause a more rapid combination of the gas with the water, in the same manner as is done to promote the solution of carbonic acid or sulphurated hydrogen gases.

\* *Annales de Chimie*, XXXV. 225.



By the close adhesion of my finger to the mouth of the decanter, I soon perceived that a considerable vacuum was produced, or, in other words, that a great portion, or perhaps even the whole of the phosphorated hydrogen gas contained in the decanter had become dissolved in with the water.

I then, not without difficulty, withdrew my finger from the orifice of the decanter, in order to examine more attentively the properties of the fluid it contained, as well as to discover whether some particles of gas, capable of inflammation by the contact of air, might not remain. But scarcely had the atmospheric air entered the decanter, when a strong explosion took place, attended with a very brilliant light. From this effect I was well convinced that the whole of the phosphorated hydrogen gas had not been absorbed by the water contained in the decanter, and therefore I suddenly closed the orifice, to prevent the continuance of the combustion, which would certainly have produced a considerable quantity of phosphoric acid, and, consequently, have produced a degree of uncertainty in the result of my enquiries.

The decanter being exactly closed, I again shook it several times, from a notion that I should thus succeed in completely fixing the last portions of hydrogen gas, which still remained undissolved.

In hopes of success, and impatient to ascertain the new properties which might have been acquired by the impregnated water, I resolved to open the bottle a second time, and expose its contents to the contact of the air. Another explosion soon followed, but less violent than the first. After this event, I did not again close the bottle, but observed that an extremely pale flame continued to escape from it during several minutes.

When there no longer remained any apparent signs of combustion, I proceeded to smell and taste the fluid in the decanter. Its smell was extremely disagreeable, and very different from that afforded by gaseous phosphorated hydrogen: and its taste, though very bitter, was nevertheless insipid and nauseous. Its colour inclined to yellow.

With the tincture of turnsol it soon became perceptibly red, which I attributed to the small portion of phosphoric acid produced at the moment when the explosions took place, as well as during the time of the gradual inflammation which succeeded the second detonation.

I was now, therefore, obliged to repeat the operation, by dissolving some phosphorated hydrogen a second time in water, in order to ascertain, by a greater attention to my experiment, in what exact proportion this solution might be effected, and also to prevent the combustion from taking place in the vessel: otherwise my trials would have remained inconclusive.

*(To be concluded in our next.)*

*SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.**Premiums offered by the Board of Agriculture.*

**T**HIS Board having been required by a committee of the House of Lords, "to examine into, and report to their lordships, the best means of converting certain portions of grass lands into tillage, without exhausting the soil, and of returning the same to grass, after a certain period, in an improved state, or at least without injury," and being desirous that their information, on a subject of so great importance, should be complete—adapted to every sort of soil, and founded on the most ample experience—have come to the resolution of offering the following premiums for that purpose, viz.

To the person who shall produce, on or before the first day of February, 1801, the best and most satisfactory essay on the subject before mentioned, distinguishing respectively, what part of the plan recommended, or of the details given, is the result of actual experiment, accurate observation, or well-authenticated information—*Two Hundred Pounds.*

For the second best—*One Hundred Pounds.*

For the third best—*Sixty Pounds.*

For the fourth best—*Forty Pounds.*

And to such persons who may communicate information, which, though useful, may be considered of less importance, smaller rewards, proportioned to the opinion of the Board.

It is required that each essay shall fully detail the course of crops, regard being had to the varieties of soil, and the time proposed for continuing the land under tillage.

Also, to explain the cases in which it may be eligible to drain land, previous to tillage.

In what cases paring and burning are advantageous, with directions thereon, regard being had to the subsequent cropping.

The depth to which grass lands should, at first breaking up, be ploughed.

Whether the crops, intended for cattle and sheep, are to be fed on the land, and by which kind of stock, or carted off.

To state—

The crop with which the grass seeds, in each case, ought to be sown, when the land shall be again laid down:

The sorts and quantities of grass seeds for each kind of soil, and whether to be provided by landlord or tenant:

Whether it be best to mow or feed the grass in the first year after laying down—to detail the management in each case:

The manuring which may be thought necessary:



The principle on which an increase of rent ought to be estimated, where permission may be given to break up old pasture now under lease.

The Board requires that these objects should be particularly attended to, with relation to the leading qualities of land, viz.

Clay, in all its distinctions; and soils too strong or wet for turnips:

Loam, in all its distinctions, fit for turnips:

Sand, including warrens and heaths, as well as rich sands:

Chalk land, and downs:

Peat, including moory, feggy, rough bottoms, and fens.

It is hoped that no person will be deterred from communicating his knowledge to the Board, on account of his experience being confined to one of these soils only.

The Board reserves the power of withholding any premium, in the case of no essay being deemed sufficiently important to merit it.

The essays which shall obtain any premium, or other reward, to remain the property of the Board.

Each essay to be sent (sealed) without a name, but with a mark, or motto; and accompanied by a sealed letter with the same mark or motto, containing the name and address of the author; and this letter will not be opened, unless one of the prizes, or some other reward, shall be adjudged to him.

All communications to be addressed to Lord Carrington, President, Sackville-street.

*Prizes offered by the Class of Mathematical and Physical Sciences of the National Institute of France in its Public Sitting, 15th Germinal in the year 8. (April 4, 1800.)*

#### *The first Prize.*

The class of Mathematical and Physical Sciences having proposed a second time in the year 6, as the subject of a prize to be determined at the public sitting of Germinal in the year 8, the anatomical comparison of the liver in the different classes of animals; and no memoir having been transmitted to them on this subject, the class has thought proper to withdraw it, and propose the following question:

*It is required to determine by anatomical and chemical experiments, what are the phenomena of the torpid state which certain animals, such as the dormouse, the marmot, &c. experience during the winter, with regard to the circulation of the blood, respiration, and irritability: to ascertain what are the causes of this sleep, and why it is peculiar to those animals.*

The candidates are invited to examine more particularly into the differences presented by those animals in their lethargic and their ordinary states, with regard to the pulse, the heat of the blood, the frequency of respiration, the quantity of oxygen consumed in a given

given time, and the excitability by galvanism. They will also examine the anatomical differences which distinguish these animals, from such as are not subject to become torpid in the winter, and enquire how far these differences may be sufficient to explain the phenomena of this lethargic state.

The prize, which is a gold medal of the value of one kilogramme (15450 grains, or about 128l. sterling) will be given at the public sitting of the 15th Germinal, in the year 11 (April 4, 1803.)

The memoirs will be received till the 15th Messidor in the year 10, and not afterwards, (July 2d. 1802.)

#### *The second Prize.*

The same class proposed in the year 6, as the subject of a prize to be determined at the public meeting of Germinal in the year 8, the following question:

*To ascertain by accurate experiment the influence of atmospheric air, of light, of water, and of earth in vegetation.*

Though the institution has received no memoir on this interesting question, it has thought proper to propose it again; but as the different works which are necessary for the complete solution require much time and many experiments, and it can scarcely be expected, that all the elucidation which is desired can be obtained from the candidates during the intended prolongation of the term;

The class has determined, that in case no memoir should be received at this second concurrence, in which the question shall have been treated in its utmost extent, the prize shall be given to that piece which shall contain only a series of experiments, facts, and observations, which shall appear the most proper to increase the information we already possess respecting some parts of the problem proposed.

The prize will be a gold medal of the value of one kilogramme (15450 grains, or about 128l. sterling.) It will be given at the public sitting 15 Messidor, year 10, (July 2, 1802.) The memoirs must be sent before the first Nivose in the year 10, (Dec. 21, 1801.)

#### *Third Prize.*

*What are the characters which distinguish among vegetable and animal substances, those which serve as ferments, from those in which they excite the fermentative process?*

The prize will be a gold medal of the value of a kilogramme, and it will be distributed at the public sitting of the 15 Germinal in the year 10.

The memoirs must be sent before the first Nivose in the same year.



*Candles with wooden Wicks.*

Professor Medicus of Heidelberg has given an account of the candles with wooden wicks, which for some years past have been manufactured and used at Munich, from the original instructions of Count Rumford, whose active services in advancing the comforts of social life are sufficiently known\*. The professor used them for a whole winter, and found them superior to every other kind of candle in the following advantages: they give the same quantity of light as a wax candle (bougie); they burn with an equal and constant flame; they do not sputter, and never run over.

The only difference between these and common candles consists in the wick. The professor did not know the actual process of the manufactory in wrapping the cotton round the wood. The editors of the *Annales* think, that the flake of cotton delivered from the carding engine, and then pressed between rollers, would be exceedingly well adapted to the purpose; and there is no doubt that various methods may be adopted for so easy a process. Any kind of resinous wood may be used, and tallow of the first quality is applied to this object in Bavaria. The candles are sold one eighth dearer; but as it is stated that they last one fourth longer, there must be a considerable saving.

The size of the wood is about that of a straw, and the pieces are dipped in wax or tallow, so as to have a very slight covering of the material. They are then rolled up in the carded cotton till they have acquired the size of a common wick. The covering must be very equally distributed. The editors propose, in order to detach the wick from the rest of the cotton without the use of scissors, that a straight edge of iron should be pressed on the place where the separation is intended, and the wick then drawn away.

The candles are made in a mould as usual, taking care to place the wick exactly in the middle. Some experiments would no doubt require to be made, particularly to ascertain the best dimensions or proportions of wood, cotton, and tallow, for candles of different sizes. The snuffers would require to be sharper than for cotton wicks. I do not remember how we snuffed the lob chock at China; but I think we broke off the charred piece with the fingers, or any convenient implement.

*Ink capable of resisting the Action of Oxygenated Muriatic Acid.*

Since the discovery of the bleaching power of the oxygenated muriatic acid, and particularly that of discharging the traces of common ink, it has become an object of serious investigation to form an atramentous fluid, which should not be subject to a treatment so

\* The account is in Reim's *Neue Fortgesetzte Sammlung Vermischter Oekonomischen Schriften*. 12. Th.---I have not this work, but recur to the *Annales des Arts et Manufactures* II. 100. I do not know whether the Count was led to this construction by his own meditations, or from a knowledge of the Chinese candle, called the *Lob chock*, for an account of which see our first Vol. page 72.---N.

obviously inimical to the written evidence of the most important contracts in human society. Many German chemists have made experiments with their usual intelligence and skill to resolve this problem. Among the receipts for this purpose the following are the principal\*:

1. By Westrumb. Boil one part of Brazil wood, and three parts of pounded nut galls, with forty-six parts (the whole by weight) of pure water. When the liquid is reduced to thirty-two parts, pass it through a strainer while hot upon one part and half of very pure sulphate of iron, one part and quarter of gum arabic, and a quarter of a part of fine sugar. When all these substances are dissolved, add one part and a quarter of good indigo ground as fine as possible, and three quarters of a part of purified lamp black.

As the indigo is the ingredient in this ink which resists the oxygenated acid, and as it is only mechanically suspended, it is of importance that the fluid should be stirred well every time it is used, lest the black colour of the fluid should deceive the writer.

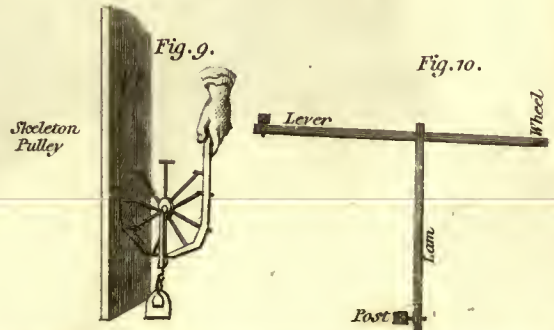
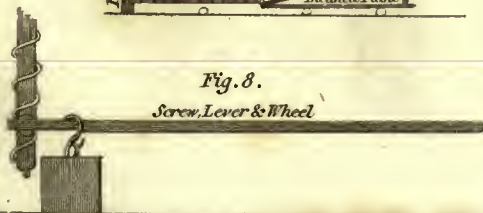
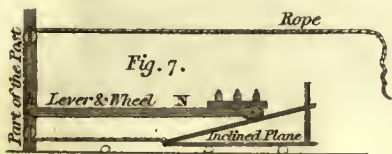
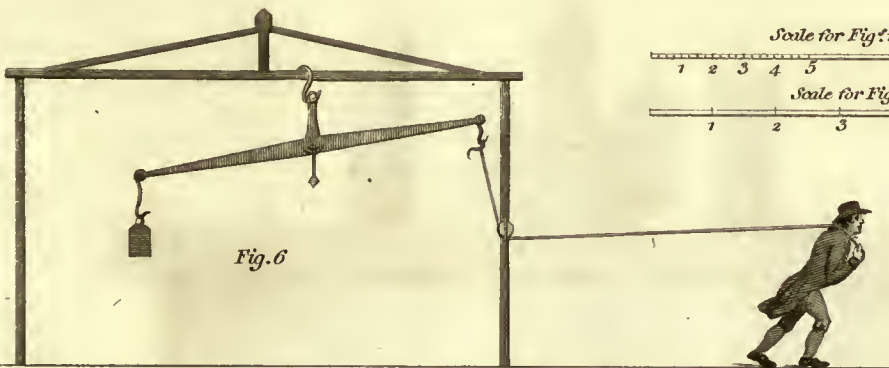
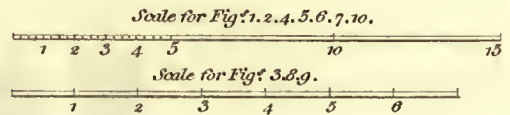
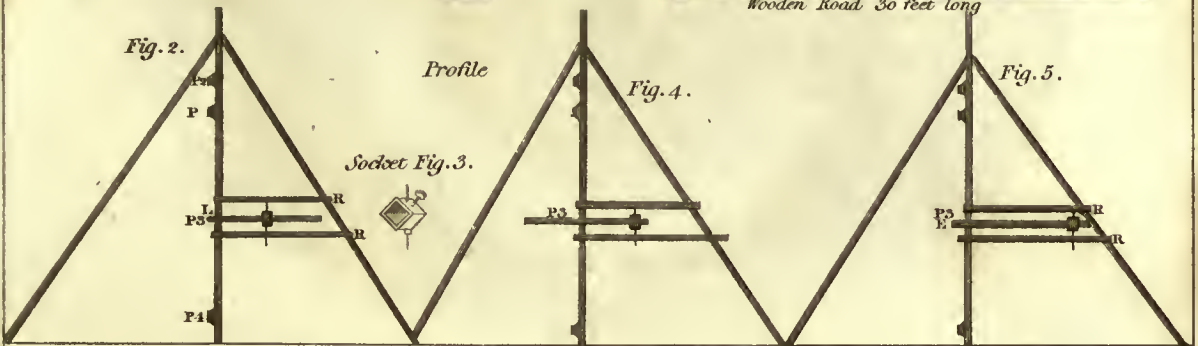
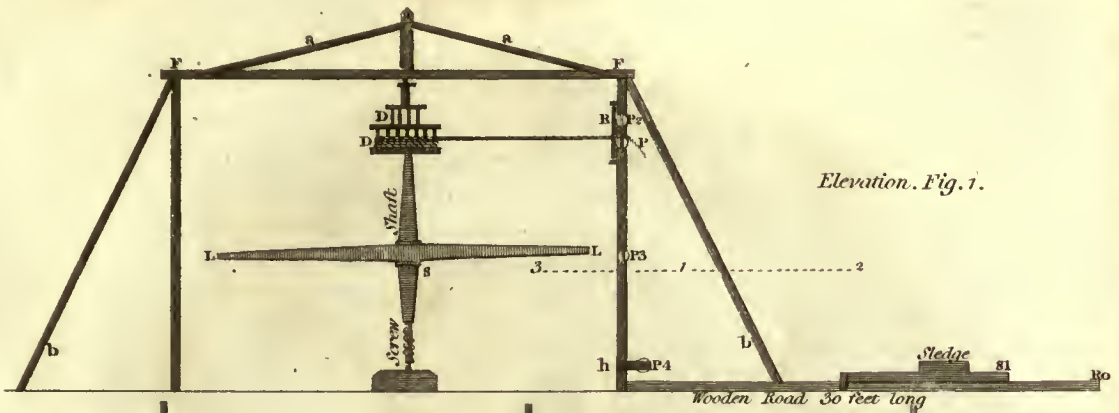
2. By Boffe. Boil for about a quarter of an hour, one part by weight of Brazil wood, with twelve parts of water. Add half a part of allum, evaporate the whole to eight parts, and mix one ounce (q. part?) of manganese, levigated to a very fine powder, and half a part of powdered gum arabic.

3. Apparently by the editor of the *Annales*. Boil for eight minutes one part by weight of Brazil wood, with three parts of powdered nut galls in nine parts of vinegar, and an equal quantity of water. After straining the liquid, dissolve therein half a part of sulphate of iron, and one part of gum arabic, after which add a solution of half a part of indigo in one part of concentrated sulphuric acid. The oxygenated muriatic acid, if applied, will dissolve the oxide of iron in this ink, but it cannot decompose the indigo, and the great object is to prepare the ink in such a manner, as that it shall not be discharged by that means.

4. The same editor also observes, that the common ink may be rendered incapable of being completely discharged by any action which the paper can withstand, if instead of water, or other liquids, the expressed juice of green vegetables be used; such for example as the lathiris linn. the sambucus niger, or common grafs.

\* *Annales des Arts et Manufactures* II, 106.

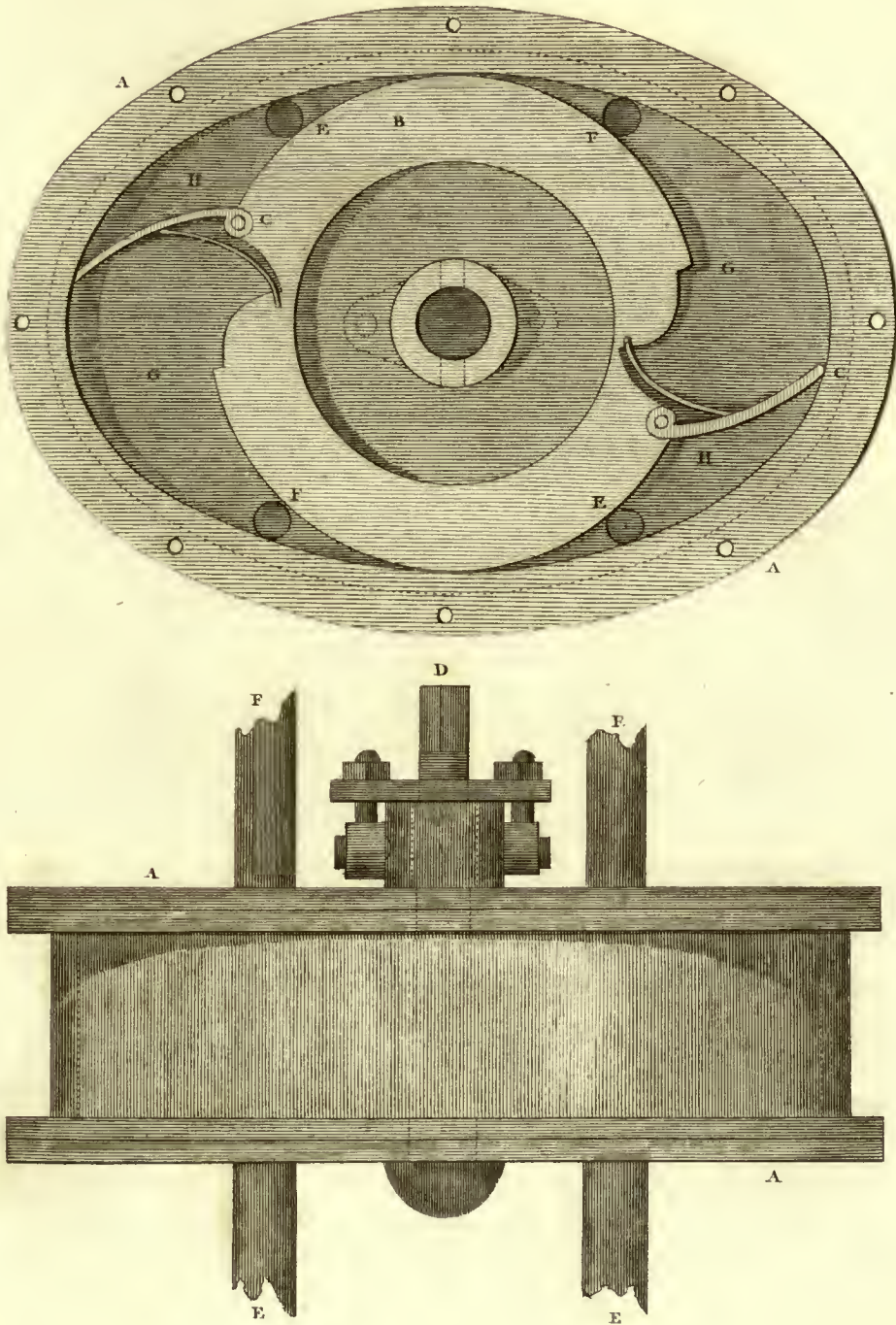








*Rotatory hydraulic Engine.*







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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

---

FEBRUARY 1801.

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ARTICLE I.

*Observations on a Method of restoring the Utility of Wells, which have been abandoned in consequence of the Mephitization of the Ground. By Cit. CADAT-DE-VAUX, of the Society of Agriculture of the Department of the Seine (in France.) Communicated at the Sitting of that Society on the 16 Brumaire, in the year 8.\**

THE well of an house in *la Rue de Beaune* was infected for seventeen years by the drainings, or transfusions from the neighbouring privies. The proprietor desiring, if possible, to restore it to the use of the inhabitants, applied to C. Dufour, who resides in *la rue traverse* f. St. Germain, and has successfully attended to that department of hydraulics which relates to springs, wells, and fountains, who obtained the desired effect by an happy application of the *artesian process* (procédé artésien.)

The artesian fountains and wells, as they are denominated in Flanders and Belgia, derive their name from the Province of Artois, where they were probably invented, and are made as follows:

\* Translated from the *Decade Philosophique*, An. VIII. 10 frimaire, p. 398.

The ground where one of these fountains or wells is intended to be made, is perforated with the borer; and it depends upon circumstances, whether the result shall be a well or a fountain, that is to say, upon the greater or less elevation of the reservoir from which the fluid is supplied.

In the perforation in the ground is placed a wooden pipe, which is driven down with a mallet, after which the boring is again continued, in order that the pipe may be driven still lower. By means of the borer, the strata of gravel, stones, and even rocks, if present, are perforated. In proportion as the cavity of the instrument becomes loaded, it is drawn out and emptied, and in time, (for this is not a very speedy operation) by the addition of new portions of wooden pipe, the boring is carried to great depths, and water is at last obtained, unless the labour shall have proved to be in vain; a thing which sometimes, though very seldom, happens.

If the reservoir, or vertical head of the water obtained, should prove higher in its level than the surface of the ground, the water springs up, and the result is not a well, but a fountain. C. Dufour has thus obtained one at Courtalin, which affords water in sufficient abundance to supply the paper manufactory of C. C. Réveillon and La Garde.

If on the contrary the level be lower, the water cannot rise above some elevation beneath the upper orifice of the cylinder. It is then an artesian well.

A fountain or spouting spring may be hoped for when the boring is performed on the side of an hill, or in a district environed by mountains or hills; but in the plains at a great distance from these natural reservoirs, it is scarcely to be expected that this operation will afford more than a well.

In those cases where no more than a metre (yard) or two is wanting to give the water that elevation which would produce a fountain spring, an excavation or basin is made to the proper depth round the pipe, which being sawed off affords a jet of water, and fills the basin, whence it may be drawn off by a bucket, or pump.

The artesian wells are preferable in every respect to our wells. They are less expensive, and the supply of water is more certain and abundant. In fact, in the common construction of our wells, when the workmen have arrived at the water, and the springs gain upon them, it becomes necessary to fix the windlafs, and too frequently a well is made which supplies but a moderate quantity of water, and is dry in summer.

In this case there is no remedy but to use the artesian process; that is to say, in order to recover the water in these dry wells, it is necessary to perforate the bottom, insert a cylindrical pipe, and proceed to seek for water at a greater depth.

This method of adding to the depth of wells already in existence, and affording a scanty supply of water, is due to an inhabitant of Thuringia. The æconomical society of Leipzig in its Collection of Memoirs (Semestre St. Michel 1785) has published this process. One of its members, who had a well which had become dry, applied the artesian process with great success. The author speaks of this method as capable of speedily affording a large quantity of water, as of great utility in a camp or fortress; and when the waters near  
the



the surface are not of a good quality, this is the best method of proceeding in search of better at a great depth, as Cit. Dufour has done in the well which forms the subject of the present observation.

After the well in question had been emptied, C. Dufour bored the bottom, and placed a pipe of ten feet in the hole. This depth was sufficient to pass through the bed of mephitized earth, and reach the lower body of water, which being thus insulated by the cylinder, rises pure into the body of the pump, which is fixed for that purpose.

I have thought it useful to give publicity to this last process, more especially with regard to Paris, where the necessaries and drains have successively infected the ground by their bad construction, and rendered a great number of wells useless. This method appears to be the only one capable of remedying this inconvenience, and restoring the utility of these abandoned wells.

I should reproach myself for not entering into a longer detail respecting these artesian wells and fountains, on account of the preference they deserve over our ordinary wells, by the advantages they present. But knowing that my colleague C. Gillet Caumont possesses very interesting facts respecting them, I have invited him, and he has promised the Society to extract from a work undertaken on this object, whatever may relate to its rural and economical uses, and to publish it\*.

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## II.

*Memoir on several newly discovered Properties of phosphorated Hydrogen Gas. By Citizen RAYMOND, Professor of Chemistry at the Central School of the Department of the Ardèche.*

*(Concluded from Page 475.)*

FROM the quantity of water and gas which I had at first attempted to dissolve, as well as from the explosions which took place the instant the bottle was unstopped, I was certain that the distilled water could not, at a temperature of twelve degrees, dissolve an equal volume of phosphorated hydrogen gas. In the second experiment, therefore, I had the precaution to fill the decanter only one third with the above gas; after which I shook the mixture several times, in order to facilitate the combination of the gas with the water, and

\* It is at present a common practice in London, since the general introduction of the steam engine into private manufactories, to sink a well, below the stratum containing the surface water, into a quicksand, where plenty of soft water is met with. The artesian process undoubtedly deserves to be better known from its simplicity, cheapness, and other good qualities. Dr. Darwin has described a somewhat similar operation in the *Philos. Transf.* Vol. LXXV. 1, and in the same work, Vol. LXXIV. may be seen the Account of the King's Wells at Harwich, by Tho. Hyde Page, Esq. and Mr. Vulliamy's overflowing well, in our Journal II. 276.---N.

at the same time to make the union as complete as possible. I then unstopped the decanter, and inverted it in a small tub, full of newly distilled water, to ascertain whether it would be entirely filled with this water, by the mere pressure of the air, and that I might also by this means be enabled to judge whether the whole of the gas had become liquefied.

I accordingly saw that a portion of the water in the tub was suddenly pressed into the decanter, but not so much as to fill it entirely, which shewed that some of the gas still remained uncombined with the water. I next made trial of the absorbed gas by passing a few bubbles through the liquid, which took fire by contact of the external air, and thus proved that the phosphorated hydrogen gas had not been at all decomposed either by the agitation, or by its contact with the distilled water; whereas it speedily loses its high degree of combustibility, when collected in vessels filled with undistilled water, or with water that had been distilled for a length of time. This effect I attribute to the quantity of air held in solution by common water, the oxygen of which coming in contact with a portion of phosphorus separates it from the hydrogen, by converting it into phosphoreous oxide, which being perfectly insoluble, is deposited on the inside of the glass, without affording any perceptible combustion in this kind of oxidation; because the oxygen of the atmospheric air, which partakes of the liquidity of water, being thus deprived of a great part of the caloric and light combined with it, during its aerial aggregation, cannot produce both those effects in a perceptible manner, when it passes from one combination to another in this state of liquid aggregation.

Having again shaken the decanter, after closing its mouth, I succeeded by means of the small quantity of water which had entered, and of which I kept an accurate account in obtaining the absorption of the rest of the gas; for on opening the vessel again, it soon became entirely filled with water. Hence I may venture to affirm, from the diversified trials, as well as from several others which I shall pass over, that water which has been deprived of its air by distillation, is capable of dissolving and liquifying at the ordinary temperature, rather less than one fourth of its bulk of phosphorated hydrogen gas, and that by this proportion it is completely saturated.

This solution, prepared according to the above directions, and preserved from the contact of air, has always exhibited the following properties: its colour is nearly similar to that of roll brimstone, though in general pale. Its smell is strong and disagreeable, and its taste extremely bitter, though insipid and nauseous.

On examining this solution in the dark, it did not appear in the least luminous, a proof that the phosphorus had intimately combined with the hydrogen,

When distilled in a small retort with the pneumato-chemical apparatus, a little below the temperature of boiling water, and particularly if submitted when recently prepared, it afforded a considerable quantity of phosphorated hydrogen gas, equally pure and combustible as that obtained by heating caustic alkalies, or quick lime with phosphorus, and the addition of a small quantity of water. What remained in the retort after the disengagement



ment of the gas had entirely ceased, was nothing but pure water, without smell, taste, or colour; and in every respect similar to that newly distilled.

When exposed to the air of the atmosphere, it soon deposited a remarkable quantity of red oxide of phosphorus, and at the same time emitted some hydrogen gas, but which was no longer susceptible of inflammation, except by the contact of an ignited body. By keeping the solution exposed to the air for a length of time, and occasionally increasing or renewing the points in contact by a gentle agitation, it becomes completely decomposed, or in other words, it resolves itself entirely into phosphoreous oxide, and pure hydrogen gas.

The tinctures of turnsol and violets do not undergo any change in their colour by contact with the liquid phosphorated hydrogen, a proof that this fluid is neither acid nor alkaline.

The sulphuric and nitric acids, and the muriatic acid either simple or oxygenated, on being added to this liquor, produced no remarkable effect; neither did the application of potash, soda, nor ammonia.

The oxydes of mercury and lead were speedily reduced, and immediately converted into metallic phosphurets, by their mixture with the solution of phosphorated hydrogen gas.

When poured into the nitrate of silver this solution instantly produces a black precipitate in great abundance, which does not change its colour, and which before the blow pipe exhibits all the characters of metallic phosphurets.

When brought into contact with the nitric solution of mercury, it also produced a very considerable precipitate in great abundance, which is at first black, but soon becomes white and crystallized, in proportion as it passes from the state of phosphuret to that of the mercurial phosphate, by absorbing oxygen either from the nitric acid in which it takes place, or the atmospheric air with which it is in contact.

The solution of lead by the nitric acid is likewise decomposed by the hydro-phosphorated liquor, but with less activity than the solutions of silver and mercury. In this decomposition some phosphuret of lead is also produced, which in time becomes converted into phosphate.

The sulphate of copper also, after a certain time, exhibits a considerable quantity of black precipitate, when a portion of phosphorated hydrogen gas is added to the solution. This precipitate, like that obtained by the decomposition of the nitrate of silver, retains its black colour; which seems to shew, that it cannot without great difficulty be converted into phosphate.

The sulphate of iron did not appear to be affected by this liquid.

Nitrate of arsenic, when poured into this fluid, does not undergo any perceptible decomposition, till the expiration of several days; but at length a precipitate is generated of a beautiful yellow colour, in the form of small grains, which is capable of remaining exposed to the air for a length of time, without undergoing any change. This precipitate is the arsenical phosphuret.

## CONCLUSIONS.

## CONCLUSIONS.

The following are the new properties which I conceive I have been the first to discover in the phosphorated hydrogen gas.

1. This gas is capable of uniting with distilled water in the proportion of about one-fourth of its bulk, when the solution is effected at about ten degrees of the French thermometer\*.

2. It communicates to the water in which it is dissolved a strong and disagreeable smell, as well as a bitter taste; which may at some future period be used with success in the treatment of various diseases, either from the facility with which this preparation is decomposed, or on account of the effect produced by the phosphorus it contains, in the formation of animal substances.

3. When water, purified of its air, is used for the purpose of dissolving this gas, and if, when thus dissolved, it is carefully preserved in closely stopped vessels, it may be kept for a long time without undergoing decomposition; so that by heating the solution, the whole of the phosphorated hydrogen gas which it contains may be disengaged in the gaseous state.

4. When water has been by this means deprived of the phosphorated gas which it had dissolved, it again returns to its original state of purity; whence it follows that its new properties are derived from the presence of this gas alone.

Lastly, this solution is capable of speedily reducing several metallic oxydes, whether alone, or dissolved by acids, and of forming with them, by double elective attraction, water and metallic phosphurets: which combinations have only hitherto been obtained in the dry way only; that is to say, by heating metals with phosphorus, or rather by the decomposition of phosphoric glass, or metallic phosphates, by means of metals and carbon.

## III.

*Description of the underground inclined Plane, executed at Walkden Moor in Lancashire, by his Grace the Duke of Bridgewater. By the Rev. FRANCIS EGERTON †.*

I BEG leave to present to the Society an account of the under-ground inclined plane, which the Duke of Bridgewater has lately made at Walkden-Moor, between Worsley and Bolton, in Lancashire. To this account I have subjoined two plans, with a table of reference to each.

\* It is probable that at the temperature of zero, a greater quantity might be dissolved by the water; but for want of a sufficient quantity of phosphorus, I was not able to ascertain this fact.

† Extracted from the Transactions of the Society for the Encouragement of Arts, &c. for the year 1800. The letter is addressed to their secretary, and the Society has voted their annual gold medal to his Grace.



At Worsley the Duke of Bridgewater's navigation begins; it goes west to Leigh, and east to Manchester, where it locks up into the Rochdale canal. In its way to Manchester, it turns out, in a western direction, near Longford Bridge, to meet the Grand Trunk Canal, above Preston Brook; and from thence it goes north-west to Runcorn, where it locks down into the Mersey, in the tide-way to Liverpool.

To this navigation above-ground, which, in all its directions, is extended through a length of \* forty miles, upon one level, without tunnel or lock, except the locks at the extremities. At Worsley, an underground navigation is joined, which goes to the different mines of coal under Walkden-Moor; from which mines, by these navigations above-ground and under-ground, Manchester and various other places are supplied with that valuable article.

The canals of this under-ground navigation lie upon two levels, or stories.

The lower is upon the same level with the open navigation, which it joins at Worsley; and consists, in the different lines which it pursues to the different seams of coal, of near twelve miles of tunnelling.

The higher is thirty-five yards and a half perpendicular height above the level of the lower, and varies from thirty-eight to sixty-one perpendicular yards below the surface of the earth, and consists of near six miles of tunnelling.

The tunnelling of each level is ten feet four inches wide, and eight feet six inches deep; and the depth of water, three feet seven inches.

Before a communication was made by an inclined plane, the coals were discharged by hand from the boats on the higher level, and were let down the pits in tubs by an engine and a break-wheel into those upon the lower. To convey the boats themselves from the canals of the higher level into that of the lower, was the intent of making this under-ground inclined plane. By the help of this machinery, the whole business is now done at once, without discharging or damaging the coal, and at one fourth of the expence: for the boats of the higher level are bodily let down the inclined plane, and are floated from the foot of it through nearly three miles, in a strait line, of the lower level canal, into the open navigation at Worsley: and, whereas they were before obliged to be drawn up to the surface of the earth at great inconvenience and expence, to be repaired at a work-shop on Walkden-Moor, they now come of themselves, in their course of business, to be repaired at the great dock-yard at Worsley.

\* *Forty miles upon one level.*] Adding to these forty miles, nearly twelve miles of the Duke of Bridgewater's underground navigable canal, which lie upon his lower main level, and including eighteen miles of the Grand Trunk Canal betwixt the lowest lock between Middlewich and Preston-Brook, there are seventy miles of navigable canal, without a lock, upon one level, eighty-two feet above low-water mark; whereby a communication is obtained between London, Liverpool, Bristol, and Hull. At this lowest lock the Grand Trunk Navigation locks down, to be upon a level with the Duke of Bridgewater's.

The place where the inclined plane is constructed, is adapted in a singular way for the purpose. There is a bed of white rock, or grit, eight yards twelve inches deep, which dips one in four, lying exactly in the direction most convenient for the communication between the two levels; which bed of rock is hollowed into a tunnel, driven upon the rise of the metals, by blasting with gunpowder, and working it down with wedges and hammers. In this tunnel, formed through a rock reaching from the lower to the higher level, the inclined plane is fixed; and, by its being in the heart of a rock, the whole workmanship can be pinned, secured, and compacted together at the top, bottom, and sides, most effectually:—an advantage which no inclined plane above-ground can have, and which renders this a singular production, no where perhaps to be imitated.

The run of the inclined plane is one hundred and fifty-one yards, besides eighteen yards, the length of the locks, at the north or upper end: and the fall is one in four, corresponding with the dip of the rock.

Of these one hundred and fifty-one yards, about ninety-four yards are formed into a double waggon-way, in order to let two boats, namely, the empty and the loaded boat, pass up and down; and are divided by a brick wall, supporting the roof, in which are openings for a person to escape out of the way of the boats; which double-waggon way joins in one, about fifty-seven yards from the lower level.

The whole width of the double waggon-way is nineteen feet; and of the single waggon-way, after the junction, ten feet.

These waggon-ways are supplied with iron rails, or gullies, laid on sleepers, down the whole run; and the height of the roof, above the iron rails, is eight feet.

At the top of the inclined plane there is a double lock, or rather two locks, side by side, formed in the heart of the same rock, which deliver the loaded boats from the higher level down the inclined plane, and receive the empty boats from the lower. The length of that part of the tunnel in which these are formed, is eighteen yards; the width or diameter, twenty feet six inches; and the height of the roof, at the north end and above the locks, at *d d*, *Plate XXI. Fig. 1.* twenty-one feet, to admit the break-wheel.

The bottom, or south end of the inclined plane, is six feet nine inches under the surface of the water, where the loaded boat floats off the carriage upon the canal of the lower level.

The depth of the locks, under water, at the north end, is four feet six inches; at the south end it is eight feet.

The wall between the locks is nine inches above the surface of the level water; its breadth is three feet.

The diameter of the horizontal main shaft, upon which the rope works to let the loaded boats down, and to draw the empty boats up, is four feet eleven inches, and its circumference is fifteen feet five inches. The main-rope is two inches and a half in diameter, and seven inches and a half in circumference. It is lapped round with a small cord of about an inch in circumference, for the length of about one hundred and five yards, to prevent its wearing, which it does chiefly when it drags upon the bottom, when at work,

at



at the place where the waggon-ways unite; and, for the same purpose, rollers of eight inches diameter are fixed at intervals down the run of the inclined plane. Moreover, a hollow cast-iron roller of eight inches and a half diameter is fixed across the west lock, parallel to the upper west lock-gate, and near the north end of the lock, but half a yard higher than the gate, in order to bear up the rope, and to prevent it from swagging.

A hold-fast rope is fastened to the main-rope, to stay each boat upon its waggon, as they go up or down. It is marked *kk*, in *Fig. 2, Plate XXI.* and its uses are more particularly detailed in the table of reference, at *kk*, to that plate.

Upon this horizontal main-shaft is a break-wheel above mentioned, which regulates the motion of the loaded boat going down the inclined plane.

The number of iron teeth, or cogs, in the spur-wheel, which is fastened to the side of the break-wheel, is three hundred and seventy-two; and the little nut-wheel, *No. 3, Fig. 4.* which sets it in motion, contains eleven teeth, or cogs. The nut-wheel is supported by two uprights from the pillar to the roof, and works between them. Two winches or handles, *No. 4 4, Fig. 4.* on its axis, put the main-shaft, *dd, Fig. 2.* or *No. 1, Fig. 4.* in motion. The power of both united enables a man, who uses a force equal to forty pounds weight, to set forward two tons upon the waggon-road: and this force, multiplied at the winches or handles, may be used to set forward the loaded boat out of one lock, and to bring the empty boat into the other. The boats being thus put in motion, the little nut-wheel is disengaged from the main-shaft, by a slide drawing the little nut sideways, so as to disengage the teeth, or cogs, from the cogs of the spur-wheel. The weight of four tons going down bring up about one.

The spur-wheel, however, which is fastened to the break-wheel, *No. 2, Fig. 4.* is seldom used, as it is occasionally only put in motion to regulate the stretch of the ropes when new, and to draw the light boat into the lock, when, at any time, it may happen to be over-weighted with materials, such as mortar, props, slabs, &c. for the use of the higher level collieries, and will not move of itself, upon a balance, out of the lower level.

The length of the carriage, or cradle, is thirty feet; its width is seven feet four inches. It moves upon four solid cast-iron rollers, which run upon cast-iron plates; on one side of each of which there are iron crests, which stand two inches higher than the plates, and prevent the carriage from running off the road.

The weight of neat coal, contained in the loaded boat, is about twelve tons: the boat weighs about four tons; and the carriage, or cradle, in which the boat is placed, when conveyed down the inclined plane, is about five tons:—in all about twenty-one tons.

At this inclined plane thirty loaded boats are now let down, with ease, in about eight hours; that is to say, four boats are let down in a little more than an hour. The boats used in these collieries are of different sizes and dimensions; some will carry seven, some eight and a half, some twelve tons.

The weight of neat coal, independently of the weight of the carriage and boats which is let down the inclined plane, in twelve-ton boats, in eight hours, will consequently be three

hundred and sixty tons. The weight of the carriage, suppose five tons, let down in the same time, will be one hundred and fifty tons; and the weight of the boat, suppose four tons, thirty times down, in eight hours, will be one hundred and twenty tons:—in all six hundred and thirty tons down in eight hours.

The weight of the carriage thirty times up, and thirty boats up, in eight hours, will be

Carriage, at 5 tons, 30 times up = 150 tons

Boat, at 4 tons, 30 times up = 120 tons

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In all 270 tons

up in eight hours.

So that there will be 630 tons down

270 tons up

---

In all 900 tons moved at the inclined plane, in 8 hours, exclusive of an indeterminate quantity of materials occasionally brought up for the use of the higher level collieries.

The various feeders which are loosened by opening the coals in the higher level collieries, as well as three sufficient reservoirs, which may occasionally be resorted to, and used in a dry season, keep the higher level always to its height, and afford a constant supply of water to fill the locks, for the purpose of working the inclined plane.

This inclined plane was begun in September, 1795; it was finished, and in use, in October, 1797.

Of this, as of most of his other great works, the Duke of Bridgewater was himself the planner and contriver:—to project greatly, and to execute completely, are the perfection of genius.

The singularity of the place in which it is constructed; the original boldness of the design; the ingenuity and mechanism displayed in planning and executing it; the dispatch with which it has been finished; the simplicity, beauty, and harmony of its parts, tending to one united whole; and, above all, the perfection to which it is proved to have been brought, now that it is practically in use; render it equally astonishing with any other of the stupendous works which have been so ably planned, and so successfully executed, by the first projector and patriotic father of inland navigation.

I have the honour to be, &c.

FRANCIS H. EGERTON.

*Bridgewater-House, March 5, 1800.*

*Reference*



*Reference to Plate XXI. being a Plan and Section of his Grace the DUKE OF BRIDGEWATER'S Under-ground Inclined Plane, at Walkden-Moor Colliery, near Worsley, in Lancashire.*

*Figure 2.*

*a to b*, Dip of the metals and waggon-road on the under-ground inclined plane. From *b*, on the lower level, to the mouth of the tunnel, is three miles.

*A*, The east lock.

*B*, The west lock.

*C*, Represents a section of the lock; the dotted line shows the horizontal depth, and the black line under it, the slope upon which the waggon-wheels run to receive the loaded boat, or to bring the empty boat into the lock.

*dd*, The main-shaft, four feet eleven inches diameter, upon which the ropes work to wind the boats up and down; and here also the break-wheel is fastened on, together with a spur-wheel, and a nut-wheel. See *Fig. 4. No. 1.*

*e*, A passage betwixt the higher level and the locks.

*ff*, A loaded boat going down, and an empty boat going up the under-ground inclined plane.

*G*, A brick wall from the sole to the top of the inclined plane, in order to give additional support to the roof.

*h h h h*, Openings through the brick wall *G*, into which a person may step out of the way of the boats, at the time they are passing up and down.

*i*, A bell, which is rung by the rope dotted to *b*, upon the lower level, at the bottom of the under-ground inclined plane, to give notice when the empty boat is upon the waggon, or cradle, and when the men below are ready, that the loaded boat may be let down by the men above.

*kk*, Holdfast-ropes fastened to the main-ropes. and hooked on to a ring at the south end of each boat, as it goes up or down, in order to stay the boats upon the waggon or cradle, that they may not swag, or slip off. These holdfast-ropes are spliced on to the end of the main-ropes, and run above and between the two bridle-ropes when they are fastened to the iron uprights, which are upon each side of the waggons, or cradles: and they run over the north end of the boat, to be hooked on to the south end.

*ll*, The bridle-ropes fastened to the main-ropes at *O*, and secured to two iron uprights upon each side of the waggon, or cradle.

*OO*, The places where the main-ropes, the bridle-ropes, and the holdfast-ropes, are fastened all together.

*No. 1.* An open space driven into the side of the lock *A*, to which a pit is sunk from the higher level, in order to convey the water out of the locks down to the lower level, and also to force a current of fresh air into the lower level collieries.

No. 2. A paddle to let the water out of the lock *A*, into the pit No. 1.

No. 3. A paddle to let the water out of the lock *B*, through a culvert, represented by dotted lines, under the lock *A*, into the pit No. 1.

No. 7, 7. Paddles in the lock-gates, to let the water out of the higher level into the locks.

No. 8, 8. The two north lock-gates, one to each lock, which turn upon the heels of the gates, and swing round when they are opened or shut.

No. 10, 10. Two stops or cloughs, one to each lock, which serve as lock-gates to the south end, and are raised and let down by a windlass.

*S*, A stop, which is used occasionally when the lock-gates want repairing.

*T*, The place where the boats which are to pass to or from the lower single waggon-way are directed, at pleasure, into either part of the double waggon-way, by a moveable iron sleeper or plate at that point, upon which sleeper or plate the wheels of the boat-carriage or cradle run.

*Table of Reference to Plate XXI. Fig. 4, of his Grace the DUKE OF BRIDGEWATER'S Under-ground Inclined Plane, at Walkden-Moor Colliery, near Worsley, in Lancashire.*

1. Main-shaft, on which the rope laps.
2. Break-wheel, on one side of which the spur-wheel is fastened.
3. Nut-wheel, out of gear, but which slides into the spur-wheel, when used to draw the empty boat into the lock occasionally, and which is supported by two uprights from the pillar to the roof.
- 4, 4. Winches or handles, to work the nut and spur-wheel.
- 5, 5. The main-ropes fastened to the boats, and which are lapped to prevent their wearing.
6. The spur-wheel, which is fastened on one side of the break-wheel; and on which break-wheel is a strong iron-jointed timber brace, which, according to the pressure given thereto by the man who attends it, will allow the loaded boat to descend quick or slow, or detain it in its passage.
- 7, 7. Paddles in the lock-gates, to let the water out of the higher level into the lock.
8. A hollow cast-iron roller, to prevent the main-ropes from swagging.
9. Shroud-wheel, to prevent the ropes going over the end of the main-shaft, slipping off, jerking, or breaking. This stands three inches above the main-shaft.



## IV.

*Concerning the Engine for raising Water by the lateral Communication of Motion. In a Letter from the Inventor, Mr. WILLIAM CLOSE.*

## LETTER. III.

TO MR. NICHOLSON.

S I R,

*Dalton, Jan. 12, 1801.*

SINCE the date of my second letter on raising water by the lateral communication of motion, I have occasionally turned my thoughts to the subject, and made some improvement in the plan of the apparatus for producing a series of elevations, which I shall communicate in the present letter.

The height to which the lateral communication of motion will cause the pressure of the atmosphere to raise water in one column, depends upon the height of the water in the reservoir above the conical tube. When the height of the charge is five feet, the straightest diameter of the conical tube equal to six inches, and its other dimensions adapted to the pressure; the extent of the column may be estimated at eleven feet. Under similar circumstances, when water is to be raised to the height of twenty-three feet, the following method may be pursued.

Raise the water four times by the pressure of the atmosphere, through four vertical tubes, each rising to the height of seven feet and an half above the brim of the cistern from which it is to receive water, as is represented in Fig. 3, Pl. XIII. When the charge of the reservoir is five feet, it will be of no use to make the tubes much longer than seven feet and an half, as the ascent through the space above will be slow, and the extension of the elevation in this way will seldom compensate for the loss of time.

Fig. 1, in Pl. XXI. represents the first vessel above the reservoir; C part of the water tube, and E part of the air pipe which leads to the conical tube.

Let the air pipe E be made four times the width that would be required for the elevation of one column; and let the valve represented by *m* be adopted to its top.

The air pipe represented by G should be made wide for this vessel; in all those above it may be omitted.

Into the top of the vessel D insert three small air pipes O P Q, and carry each of them to a different vessel on the top of a water tube. The length of each of these above the cistern from which it is to raise a column of water, must rather exceed the length of the lowest rarefying tube above the lowest reservoir. They will require no valves.

When the rest of the apparatus is completed upon the principles before laid down, the lateral communication of motion will rarefy the air equally in all its parts, and the water will ascend through all the vertical tubes, into and nearly fill the vessels upon their tops.

But before it quite reaches the tops of the tubes *O P Q*, it will begin to descend in a full stream through the lowest rarefying tube *E*, which is the shortest: the lateral action now ceasing to rarefy the air, the water will rise no higher, but will remain at the same elevation above each of the cisterns, until the external valves shall open. After this the valve *m* will close the end of its tube, and prevent the diminution of the jet; the air from without will enter the lowest vessel *D*, through the tube *G*; part of it will ascend from this vessel through the empty branches *O P Q* of the rarefying tube into the vessels above; and at the same time the raised water will flow out of all the vessels into different cisterns.

If all the vessels are connected together by their air pipes, instead of each being connected to the lowest, the same equality of action will take place. In both cases, as the elevation of the water in each of the tubes must correspond with the degree of rarefaction in the air before it can begin to ascend, it is evident that the tubes will all be full at the same time, although they may contain columns of unequal height, when the air begins to rarefy: the imperfection of one or two of the valves to the water tubes can therefore be attended with little inconvenience.

If the air pipes were separately connected with the conical tube, some of the vessels might be filled with water before others: this inequality, indeed, might be removed by bringing all the tubes into one; but in either case, if the external valves were to remain too long close, the raised water would descend through the air pipes, and waste rapidly from several cisterns at once. The arrangement that has been recommended will be free from this inconvenience, however long the valves may remain shut.

By proportioning the widths of the water tubes and the capacities of the vessels upon their tops, it will be easy to raise a little more water into the first cistern, than will be delivered into the second; and a little more into the second, than will be carried into the third; and so on, in order to prevent any of the lower cisterns from becoming empty of raised water. The waste thus incurred will be very inconsiderable. If, however, more water should be raised into any of the higher cisterns than those below can long afford, it will be proper to make part of it descend through tubes for the purpose, and replenish the lower ones.

As the jet will bear the admission of some air without diminution, it is not necessary that the valve *m* should perfectly close the end of the air tube: if it did, it would not be easy to open. When a cock is used instead of this valve, it must be fixed in the tube at some depth below the surface of the water in the reservoir, and a weight hung upon an arm or lever proceeding from it, to keep open the passage through the tube. This weight will also serve to counterbalance the empty vessel which contains the syphon, and opens the valves. A cock can be attended with little inconvenience in this situation: if, after long continued use, a little water should be admitted, it will cause no perceptible diminution of power.

A second and third series of elevations may be caused by connecting more air pipes with the conical tube; but as the introduction of so many pipes into the throat would diminish

its



its width, and greatly retard the expenditure, it will be best to introduce them all through holes made in the circumference of the tube, and to take off their ends even with its inside. When water is thus raised in an additional number of columns, the water tubes should be made still shorter than was recommended before, and to prevent the water which is raised by the second series from being wasted, the rarefying tubes should be carried up to such height above each vessel, that the column may counterbalance the active pressure of the atmosphere before it arrives at the top. Recourse may be had to this method when several conical tubes are employed, as is represented in Fig. 3, Pl. XIII. In this last case, however, it will be unnecessary to make any addition to the lowest vessel, as the expenditure can only take place from the lowest cistern.

In all the arrangements that have been hitherto described, the stream through the conical tube is suffered to run one half of its time without producing any useful effect. It will, however, be of very great advantage to keep the lateral action constantly in work in rarefying the air in different parts of the apparatus. The raised water may then be let out of one set of vessels while it is rising into another set, and the water that flows out of one descending vessel, and has opened the valves of the first set, may, by being carried into a small cistern a second time, and delivered from thence by a syphon into another descending vessel, be made to open the valves of the second set.

As it may sometimes happen that all the valves may be half open, and much air be thus admitted into the stream, it is likely that cocks fixed in the rarefying tubes, and placed in water, would on this occasion be preferable to valves, particularly when the rarefying tubes can be filled with water, for then no air can descend into the jet. I think, however, that this can only be necessary when several air pipes are connected with the conical tube, and the air would enter the stream on all sides.

In this manner by the assistance of the lateral action of a stream of water through one conical tube, and the pressure of the atmosphere, an useful quantity of water may be raised to a considerable height.

I am, SIR,

Your humble servant,

WILLIAM CLOSE.

## V.

*On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations. By WILLIAM HERSCHEL, LL. D. F. R. S.\**

IT will not be difficult to shew that the power of penetrating into space by telescopes is very different from magnifying power, and that, in the construction of instruments, these two powers ought to be considered separately.

In order to conduct our present inquiry properly, it will be necessary to examine the nature of luminous bodies, and to enter into the method of vision at a distance. Therefore, to prevent the inaccuracy that would unavoidably arise from the use of terms in their common acceptation, I shall have recourse to algebraic symbols, and to such definitions as may be necessary to fix a precise meaning to some expressions which are often used in conversation, without much regard to accuracy.

By luminous bodies I mean, in the following pages, to denote such as throw out light, whatever may be the cause of it: even those that are opaque, when they are in a situation to reflect light, should be understood to be included; as objects of vision they must throw out light, and become intitled to be called luminous. However, those that shine by their own light may be called self-luminous, when there is an occasion to distinguish them.

The question will arise, whether luminous bodies scatter light in all directions equally; but, till we are more intimately acquainted with the powers which emit and reflect light, we shall probably remain ignorant on this head. I should remark, that what I mean to say, relates only to the physical points into which we may conceive the surfaces of luminous bodies to be divided; for, when we take any given luminous body in its whole construction, such as the sun or the moon, the question will assume another form, as will appear hereafter.

That light, flame, and luminous gases are penetrable to the rays of light, we know from experience; † it follows therefore, that every part of the sun's disk cannot appear equally luminous to an observer in a given situation, on account of the unequal depth of its lumi-

\* Philosophical Transactions, 1800, p. 49.

† In order to put this to a proof, I placed four candles behind a screen, at  $\frac{1}{4}$  of an inch distance from each other, so that their flames might range exactly in a line. The first of the candles was placed at the same distance from the screen, and just opposite a narrow slit, two-thirds of an inch long, and  $\frac{1}{4}$  broad. On the other side of the screen I fixed up a book, at such a distance from the slit that, when the first of the candles was lighted, the letters might not be sufficiently illuminated to become legible. Then, lighting successively the second, third, and fourth candles, I found the letters gradually more illuminated, so that at last I could read them with great facility; and, by the arrangement of the screen and candles, the light of the second, third, and fourth, could not reach the book, without penetrating the flames of those that were placed before them.



nous atmosphere in different places\*. This regards only bodies that are self-luminous. But the greatest inequalities in the brightness of luminous bodies in general, will undoubtedly be owing to their natural texture; which may be extremely various, with regard to their power of throwing out light more or less copiously.

Brightness, I ascribe to bodies that throw out light; and those that throw out most are the brightest.

It will now be necessary to establish certain expressions for brightness in different circumstances.

In the first place, let us suppose a luminous surface throwing out light, and let the whole quantity of light thrown out by it be called  $L$ .

Now, since every part of this surface throws out light, let us suppose it divided into a number of luminous physical points, denoted by  $N$ .

If the copiousness of the emission of light from every physical point of the luminous surface were equal, it might in general be denoted by  $c$ ; but, as that is most probably never the case, I make  $C$  stand for the mean copiousness of light thrown out from all the physical points of a luminous object. This may be found in the following manner: let  $c$  express the copiousness of emitting light, of any number of physical points that agree in this respect; and let the number of these points be  $n$ . Let the copiousness of emission of another number of points be  $c'$ , and their number  $n'$ . And if, in the same manner, other degrees of copiousness be called  $c^2, c^3, \&c.$  and their numbers be denoted by  $n^2, n^3, \&c.$  then will the sum of every set of points, multiplied by their respective copiousness of emitting light, give us the quantity of light thrown out by the whole luminous body. That is,  $L = cn + c'n' + c^2n^2, \&c.$ ; and the mean copiousness of emitting light, of each physical point, will be expressed by  $\frac{cn + c'n' + c^2n^2, \&c.}{N} = C$ .

It is evident that the mean power, or copiousness of throwing out light, of every physical point in the luminous surface, multiplied by the number of points, must give us the whole power of throwing out light, of the luminous body. That is  $CN = L$ .

I ought now to answer an objection that may be made to this theory. Light, as has been stated, is transparent; and, since the light of a point behind the surface of a flame will pass through the surface, ought we not to take in its depth, as well as its superficial dimensions? In answer to this, I recur to what has been said with regard to the different powers of throwing out light, of the points of a luminous surface. For, as light must be finally emitted through the surface, it is but referring all light arising from the emission of points behind the surface, to the surface itself, and the account of emitted light will be equally true. And this will also explain why it has been stated as probable, that different parts of the same luminous surface may throw out different quantities of light.

\* See the Paper on the Nature and Construction of the Sun. Phil. Transf. for 1795, page 46.

Since, therefore, the quantity of light thrown out by any luminous body is truly represented by  $CN$ , and that an object is bright in consequence of light thrown out, we may say that brightness is truly defined by  $CN$ . If however, there should at any time be occasion for distinction, the brightness arising from the great value of  $C$ , may be called the intrinsic brightness; and that arising from the great value of  $N$ , the aggregate brightness; but the absolute brightness, in all cases, will still be defined by  $CN$ .

Hitherto we have only considered luminous objects, and their condition with regard to throwing out light. We proceed now to find an expression for their appearance at any assigned distance; and here it will be proper to leave out of the account, every part of  $CN$  which is not applied for the purpose of vision.  $L$  representing the whole quantity of light thrown out by  $CN$ , we shall denote that part of it which is used in vision, either by the eye or by the telescope,  $l$ . This will render the conclusions that may be drawn hereafter more unexceptionable; for, the quantity of light  $l$  being scattered over a small space in proportion to  $L$ , it may reasonably be looked upon as more uniform in its texture; and no scruples about its inequality will take place. The equation of light, in this present sense, therefore, is  $CN = l$ .

Now, since we know that the density of light decreases in the ratio of the squares of the distances of the luminous objects, the expression for its quantity at the distance of the observer  $D$ , will be  $\frac{l}{D^2}$ .

In natural vision, the quantity  $l$  undergoes a considerable change, by the opening and contracting of the pupil of the eye. If we call the aperture of the iris  $a$ , we find that in different persons it differs considerably. Its changes are not easily to be ascertained; but we shall not be much out in stating its variations to be chiefly between 1 and 2 tenths of an inch. Perhaps this may be supposed under-rated; for the powers of vision, in a room completely darkened, will exert themselves in a very extraordinary manner. In some experiments on light, made at Bath, in the year 1780, I have often remarked, that after staying some time in a room fitted up for these experiments, where on entering I could not perceive any one object, I was no longer at a loss, in half an hour's time, to find every thing I wanted. It is however probable that the opening of the iris is not the only cause of seeing better after remaining long in the dark; but that the tranquillity of the retina, which is not disturbed by foreign objects of vision, may render it fit to receive impressions such as otherwise would have been too faint to be perceived. This seems to be supported by telescopic vision; for it has often happened to me, in a fine winter's evening, when, at midnight, and in the absence of the moon, I have taken sweeps of the heavens, of four, five, or six hours duration, that the sensibility of the eye, in consequence of the exclusion of light from surrounding objects, by means of a black hood which I wear upon these occasions, has been very great; and it is evident, that the opening of the iris would have been of no service in these cases, on account of the diameter of the optic pencil, which, in the 20 feet telescope,



telescope, at the time of sweeping, was no more than  $\frac{1}{12}$  inch. The effect of this increased sensibility was such, that if a star of the 3d magnitude came towards the field of view, I found it necessary to withdraw the eye before its entrance, in order not to injure the delicacy of vision acquired by long continuance in the dark. The transit of large stars, unless where none of the 6th or 7th magnitude could be had, have generally been declined in my sweeps, even with the 20 feet telescope. And I remember, that after a considerable sweep with the 40 feet instrument, the appearance of Sirius announced itself, at a great distance, like the dawn of the morning, and came on by degrees, increasing in brightness, till this brilliant star at last entered the field of view of the telescope, with all the splendour of the rising sun, and forced me to take the eye from that beautiful sight. Such striking effects are a sufficient proof of the great sensibility of the eye, acquired by keeping it from the light.

On taking notice, in the beginning of sweeps, of the time that passed, I found that the eye, coming from the light, required near 20', before it could be sufficiently reposed to admit a view of very delicate objects in the telescope; and that the observation of a transit of a star of the 2d or 3d magnitude, would disorder the eye again, so as to require nearly the same time for the re-establishment of its tranquillity.

The difficulty of ascertaining the greatest opening of the eye, arises from the impossibility of measuring it at the time of its extreme dilatation, which can only happen when every thing is completely dark; but, if the variation of  $a$  is not easily to be ascertained, we have, on the other hand, no difficulty to determine the quantity of light admitted through a telescope; which must depend upon the diameter of the object-glass, or mirror; for, its aperture  $A$  may at all times be had by measurement.

It follows, therefore, that the expression  $\frac{a^2 l}{D^2}$  will always be accurate for the quantity of light admitted by the eye; and that  $\frac{A^2 l}{D^2}$  will be sufficiently so for the telescope. For it must be remembered, that the aperture of the eye is also concerned in viewing with telescopes; and that, consequently, whenever the pencil of light transmitted to the eye by optical instruments exceeds the aperture of the pupil, much light must be lost. In that case, the expression  $A^2 l$  will fail; and therefore, in general, if  $m$  be the magnifying power,  $\frac{A}{m}$  ought not to exceed  $a$ .

As I have defined the brightness of an object to the eye of an observer at a distance, to be expressed by  $\frac{a^2 l}{D^2}$ , it will be necessary to answer some objections that may be made to this theory. Optical writers have proved, that an object is equally bright at all distances. It may, therefore, be maintained against me, that since a wall illuminated by the sun will appear equally bright, at whatsoever distance the observer be placed that views it, the sun also,

at the distance of Saturn, or still farther from us, must be as bright as it is in its present situation. Nay, it may be urged, that in a telescope, the different distance of stars can be of no account with regard to their brightness, and that we must consequently be able to see stars which are many thousands of times farther than Sirius from us; in short, that a star must be infinitely distant not to be seen any longer.

Now, objections such as these, which seem to be the immediate consequence of what has been demonstrated by mathematicians, and which yet apparently contradict what I assert in this paper, deserve to be thoroughly answered.

It may be remembered, that I have distinguished brightness into three different sorts\*. Two of these, which have been discriminated by *intrinsic* and *absolute* brightness, are, in common language, left without distinction. In order to shew that they are so, I might bring a variety of examples from common conversation; but, taking this for granted, it may be shewn that all the objections I have brought against my theory have their foundation in this ambiguity.

The demonstrations of opticians, with regard to what I call *intrinsic* brightness, will not oppose what I affirm of *absolute* brightness; and I shall have nothing farther to do than to shew that what mathematicians have said, must be understood to refer entirely to the intrinsic brightness, or illumination of the picture of objects on the retina of the eye; from which it will clearly follow, that their doctrine and mine are perfectly reconcilable; and that they can be at variance only when the ambiguity of the word brightness is overlooked, and objections, such as I have made, are raised, where the word brightness is used as *absolute*, when we should have kept it to the only meaning it can bear in the mathematicians' theorem.

The first objection I have mentioned is, that the sun, to an observer on Saturn, must be as bright as it is here on earth. Now by this cannot be meant, that an inhabitant standing on the planet Saturn, and looking at the sun, should *absolutely* receive as much light from it as one on earth receives when he sees it; for this would be contrary to the well known decrease of light at various distances. The objection, therefore, can only go to assert, that the picture of the sun, on the retina of the Saturnian observer, is as *intensely* illuminated as that on the retina of the terrestrial astronomer. To this I perfectly agree. But let those who would go farther, and say that therefore the sun is *absolutely* as bright to the one as to the other, remember that the sun on Saturn appears to be a hundred times less than on the earth; and that consequently, though it may there be *intrinsically* as bright, it must here be *absolutely* † an hundred times brighter.

The next objection I have to consider, relates to the fixed stars. What has been shewn in the preceding paragraph, with regard to the sun, is so intirely applicable to the stars, that it will be very easy to place this point also in its proper light. As I have assented to the demonstration of opticians with regard to the brightness of the sun, when seen at the

\* See page 497.

† See the definition of *absolute* brightness, page 497.



distance of Saturn, provided the meaning of this word be kept to the *intrinsic* illumination of the picture on the retina of an observer, I can have no hesitation to allow that the same will hold good with a star placed at any assignable distance. But I must repeat, that the light we can receive from stars is truly expressed by  $\frac{a^2 l}{D^2}$ ; and that therefore their absolute brightness must vary in the inverse ratio of the squares of their distances. Hence I am authorised to conclude, and observation abundantly confirms it, that stars cannot be seen by the naked eye, when they are more than seven or eight times farther from us than Sirius; and that they become, comparatively speaking, very soon invisible with our best instruments. It will be shewn hereafter, that the visibility of stars depends on the penetrating power of telescopes, which, I must repeat, falls indeed very short of shewing stars that are many thousands of times farther from us than Sirius; much less can we ever hope to see stars that are all but infinitely distant.

If now it be admitted that the expressions we have laid down are such as agree with well known facts, we may proceed to vision at a distance; and first with respect to the naked eye.

Here the power of penetrating into space, is not only confined by nature, but is moreover occasionally limited by the failure in brightness of luminous objects. Let us see whether astronomical observations, assisted by mathematical reasoning, can give us some idea of the general extent of natural vision. Among the reflecting luminous objects, our penetrating powers are sufficiently ascertained. From the moon we may step to Venus, to Mercury, to Mars, to Jupiter, to Saturn, and last of all to the Georgian planet. An object seen by reflected light at a greater distance than this, it has never been allowed us to perceive; and it is indeed much to be admired, that we should see borrowed illumination to the amazing distance of more than 18 hundred millions of miles; especially when that light, in coming from the sun to the planet, has to pass through an equal space, before it can be reflected, whereby it must be so enfeebled as to be above 368 times less intense on that planet than it is with us, and when probably not more than one-third part of that light can be thrown back from its disk\*.

The range of natural vision with self-luminous objects, is incomparably more extended, but less accurately to be ascertained. From our brightest luminary, the sun, we pass immediately to very distant objects; for, Sirius, Arcturus, and the rest of the stars of the first magnitude, are probably those that come next; and what their distance may be, it is well known, can only be calculated imperfectly from the doctrine of parallaxes, which places the nearest of them at least 412530 times farther from us than the sun.

\* According to Mr. Bouguer, the surface of the moon absorbs about two-thirds of the light it receives from the sun. See *Traité d'Optique*, page 122:

In order to take a second step forwards, we must enter into some preliminary considerations, which cannot but be attended with considerable uncertainty. The general supposition, that stars, at least those which seem to be promiscuously scattered, are probably one with another of a certain magnitude, being admitted, it has already been shewn in a former paper \*, that after a certain number of stars of the first magnitude have been arranged about the sun, a farther distant set will come in for the second place. The situation of these may be taken to be, one with another, at about double the distance of the former from us.

By directing our view to them, and thus penetrating one step farther into space, these stars of the second magnitude furnish us with an experiment that shews what phænomena will take place, when we receive the illumination of two very remote objects, equally bright in themselves, whereof one is at double the distance of the other. The expression for the brightness of such objects, at all distances, and with any aperture of the iris, according to our foregoing notation, will be  $\frac{a^2 l}{D^2}$ ; and a method of reducing this to an experimental investigation will be as follows :

Let us admit that  $\alpha$  Cygni,  $\beta$  Tauri, and others, are stars of the second magnitude, such as are here to be considered. We know, that in looking at them and the former, the aperture of the iris will probably undergo no change; since the difference in brightness, between Sirius, Arcturus,  $\alpha$  Cygni, and  $\beta$  Tauri, does not seem to affect the eye so as to require any alteration in the dimensions of the iris;  $a$ , therefore becomes a given quantity, and may be left out. Admitting also, that the latter of these stars are probably at double the distance of the former, we have  $D^2$  in one case four times that of the other; and the two expressions for the brightness of the stars, will be  $l$  for those of the first magnitude, and  $\frac{1}{4} l$  for those of the second.

The quantities being thus prepared, what I mean to suggest by an experiment is, that since sensations, by their nature, will not admit of being halved or quartered, we come thus to know by inspection what phænomenon will be produced by the fourth part of the light of a star of the first magnitude. In this sense, I think we must take it for granted, that a certain idea of brightness, attached to the stars which are generally denominated to be of the second magnitude, may be added to our experimental knowledge; for by this means, we

are informed what we are to understand by the expressions  $\frac{a^2 l}{\odot^2}$ ,  $\frac{a^2 l}{\text{Sirius}^2}$ ,  $\frac{a^2 l}{\beta \text{ Tauri}^2}$

We cannot wonder at the immense difference between the brightness of the sun and that of Sirius; since the two first expressions, when properly resolved, give us a ratio of brightness of more than 170 thousand millions to one; whereas the two latter, as has been shewn, give only a ratio of four to one.

\* Phil. Trans. for the year 1796, page 166, 167, 168.

† The names of the objects  $\odot$ , Sirius,  $\beta$  Tauri, are here used to express their distance from us.



What has been said will carry us, with very little addition, to the end of our unassisted power of vision to penetrate into space. We can have no other guide to lead us a third step than the same beforementioned hypothesis; in consequence of which, however, it must be acknowledged to be sufficiently probable, that the stars of the third magnitude may be placed about three times as far from us as those of the first. It has been seen, by my remarks on the comparative brightness of the stars, that I place no reliance on the classification of them into magnitudes \*; but, in the present instance, where the question is not to ascertain the precise brightness of any one star, it is quite sufficient to know that the number of the stars of the first three different magnitudes, or different brightnesses, answers, in a general way, sufficiently well to a supposed equally distant arrangement of a first, second, and third set of stars about the sun. Our third step forwards into space, may therefore very properly be said to fall on the pole-star, on  $\gamma$  Cygni,  $\epsilon$  Bootis, and all those of the same order.

As the difference, between these and the stars of the preceding order, is much less striking than that between the stars of the first and second magnitude, we also find that the ex-

pressions  $\frac{a^2 l}{\beta \text{ Tauri}^2}$ , and  $\frac{a^2 l}{\text{Polaris}^2}$ , are not in the high ratio of 4 to 1, but only as 9 to 4, or  $2\frac{1}{4}$  to 1.

Without tracing the brightness of the stars through any farther steps, I shall only remark, that the diminution of the ratios of brightness of the stars of the 4th, 5th, 6th, and 7th magnitude, seems to answer to their mathematical expressions, as well as, from the first steps we have taken, can possibly be imagined. The calculated ratio, for instance, of the brightness of a star of the 6th magnitude, to that of one of the 7th, is but little more than  $1\frac{1}{3}$  to 1; but will we find by experience, that the eye can very conveniently perceive it. At the same time, the faintness of the stars of the 7th magnitude, which require the finest nights, and the best common eyes to be perceived, gives us little room to believe that we can penetrate much farther into space, with objects of no greater brightness than stars.

But, since it may be justly observed, that in the foregoing estimation of the proportional distance of the stars, a considerable uncertainty must remain, we ought to make a proper allowance for it; and, in order to see to what extent this should go, we must make use of the experimental sensations of the ratios of brightness we have now acquired, in going step by step forward: for, numerical ratios of brightness, and sensations of them, as has been noticed before, are very different things. And since, from the foregoing considerations, it may be concluded, that as far as the 6th, 7th, or 8th magnitude, there ought to be a visible general difference between stars of one order and that of the next following, I think, from the faintness of the stars of the 7th magnitude, we are authorized to conclude, that no star, eight, nine, or at most ten times as far from us as Sirius, can possibly be perceived by the natural eye.

\* Phil. Transf. for the year 1796, page 168, 169.

The boundaries of vision, however, are not confined to single stars. Where the light of these falls short, the united lustre of sidereal systems will still be perceived. In clear nights, for instance, we may see a whitish patch in the sword-handle of Perseus\*, which contains small stars of various sizes, as may be ascertained by a telescope of a moderate power of penetrating into space. We easily see the united lustre of them, though the light of no one of the single stars could have affected the unassisted eye.

Considerably beyond the distance of the former must be the cluster discovered by Mr. Messier, in 1764; north following H. Geminorum. It contains stars much smaller than those of the former cluster; and a telescope should have a considerable penetrating power, to ascertain their brightness properly, such as my common 10-foot reflector. The night should be clear, in order to see it well with the naked eye, and it will then appear in the shape of a small nebula.

Still farther from us must be the nebula between  $\eta$  and  $\zeta$  Herculis, discovered by Dr. Halley, in 1714. The stars of it are so small that it has been called a nebula†; and has been regarded as such, till my instruments of high penetrating powers were applied to it. It requires a very clear night, and the absence of the moon, to see it with the natural eye.

Perhaps, among the farthest objects that can make an impression on the eye, when not assisted by telescopes, may be reckoned the nebula in the girdle of Andromeda, discovered by Simon Marius, in 1612. It is however not difficult to perceive it, in a clear night, on account of its great extent.

From the powers of penetrating into space by natural vision, we proceed now to that of telescopes.

It has been shewn, that brightness, or light, is to the naked eye truly represented by  $\frac{a^2 l}{D^2}$ ; in a telescope, therefore, the light admitted will be expressed by  $\frac{A^2 l}{D^2}$ . Hence it would follow, that the artificial power of penetrating into space should be to the natural one as  $A$  to  $a$ . But this proportion must be corrected by the practical deficiency in light reflected by mirrors, or transmitted through glasses; and it will in a great measure depend on the circumstances of the workmanship, materials, and construction of the telescope, how much loss of light there will be sustained.

In order to come to some determination on this subject, I made many experiments with plain mirrors, polished like my large ones, and of the same composition of metal. The method I pursued was that proposed by Mr. Bouguer, in his *Traité d'Optique*, page 16,

\* See the catalogue of a second thousand of new nebulae and clusters of stars, VI. 33, 34. Phil. Trans. Vol. LXXIX. page 251.

† In the *Connoissance des Temps* for 1783, No. 13, it is described as a nebula without stars.



fig. 3.; but I brought the mirror, during the trial, as close to the line connecting the two objects as possible; in order to render the reflected rays nearly perpendicular.

The result was, that out of 100 thousand incident rays, 67262 were returned; and therefore, if a double reflection takes place, only 45242 will be returned.

Before this light can reach the eye, it will suffer some loss in passing through the eye glass; and the amount of this I ascertained, by taking a highly polished plain glass, of nearly the usual thickness of optical glasses of small focal lengths. Then, by the method of the same author, page 21, fig. 5. I found, that out of 100 thousand incident rays, 94825 were transmitted through the glass. Hence, if two lenses be used, 89918; and, with three lenses, 85265 rays will be transmitted to the eye.

Then, by compounding, we shall have, in a telescope of my construction with one reflection, 63796 rays, out of 100 thousand, come to the eye. In the Newtonian form, with a single eye lens, 42901; and, with a double eye glass 40681 will remain for vision.

There must always remain a considerable uncertainty in the quantities here assigned; as a newly polished mirror, or one in high preservation, will give more light than another that has not those advantages. The quality of metal also will make some difference; but, if it should appear by experiments, that the metals or glasses in use will yield more or less light than here assigned, it is to be understood that the corrections must be made accordingly.

We proceed now to find a proper expression for the power of penetrating into space, that we may be enabled to compare its effects, in different telescopes, with that of the natural eye.

Since then the brightness of luminous objects is inversely as the squares of the distances, it follows, that the penetrating power must be as the square roots of the light received by the eye.

In natural vision, therefore, this power is truly expressed by  $\sqrt{a^2 l}$ ; and, since we have now also obtained a proper correction  $x$ , we must apply it to the incident light with telescopes.

In the Newtonian and other constructions where two specula are used, there will also be some loss of light on account of the interposition of the small speculum; therefore, putting  $b$  for its diameter, we have  $A^2 - b^2$  for the real incident light. This being corrected as above, will give the general expression  $\sqrt{x l \times \frac{A^2 - b^2}{a^2}}$  for the same power in telescopes. But here we are to take notice: that in refractors, and in telescopes with one reflection,  $b$  will be  $= 0$ , and therefore is to be left out.

Then, if we put natural light  $l = 1$ , and divide by  $a$ , we have the general form  $\frac{\sqrt{x \cdot \frac{A^2 - b^2}{a^2}}}{a}$  for the penetrating power of all sorts of telescopes, compared to that of the natural eye as a standard; according to any supposed aperture of the iris, and proportion of light returned by reflection, or transmitted by refraction:

In the following investigation we shall suppose  $a = 2$  tenths of an inch, as being perhaps nearly the general opening of the iris, in star-light nights, when the eye has been some moderate time in the dark. The value of the corrections for loss of light will stand as has been given before.

(To be concluded in our next.)

## VI.

*Some Observations on the Head of the Ornithorhynchus paradoxus. By EVERARD HOME, Esq. F. R. S.\**

THE specimens of this extraordinary animal which have been sent to Europe, have been deprived of the internal parts, and the skins are mostly dried, and but badly preserved. Such imperfect specimens have raised the curiosity of the naturalist, and excited the ardor of the anatomist, without satisfying their enquiries.

It was natural, under these circumstances, to reserve any observations which had been made upon this newly discovered quadruped, till the entire animal should be brought home preserved in spirit, and enable us to examine the structure of its different organs; but, finding that Professor Blumenbach has been led to believe that it was an animal without teeth, an opinion which must have arisen from the imperfect state of the specimen he examined, it appeared highly proper to do away the mistake, and lay before this learned Society, such observations respecting the head of this extraordinary animal, as I have been enabled to make.

My opportunities of examining the *Ornithorhynchus* were procured through Sir Joseph Banks; who permitted me to have drawings made from the skin of one of a very large size, and which, from having been preserved in spirit, was more perfect than any of the dried specimens.

Any general description of the beak of this animal, which is its most conspicuous peculiarity, becomes unnecessary, as the accompanying drawings will give a sufficiently correct idea of the outward appearances, to answer the present purpose.

It was not permitted to examine the head anatomically; but a smaller dried specimen, received from Sir Joseph Banks, furnished me with the following observations.

The beak of the *Ornithorhynchus*, when it is cursorily examined, appears so strongly to resemble that of the duck, as to lead to the belief of its being calculated for exactly the same purposes; it will however be found to differ materially from it, in a variety of circumstances.

The beak is found, upon examination, not to be the animal's mouth, but a part added to the mouth, and projecting beyond it.

\* Philosophical Transactions 1800.



The cavity of the mouth is situated as in other quadrupeds, and has two grinding teeth on each side, both in the upper and lower jaw; but, instead of incisor teeth, the nasal and palate bones are continued forwards, lengthening the anterior nostrils, and forming the upper part of the beak; and the two portions of the lower jaw, instead of terminating at the symphysis, where they join, become two thin plates, and are continued forwards, forming the under portion of the beak.

This structure differs materially from the bill of the duck, and indeed from the bills of all birds, since in them, the cavities of the nostrils do not extend beyond the root of the bill; and, in their lower portions, which correspond to the under jaw of quadrupeds, the edges are hard, to answer the purpose of teeth, and the middle space is hollow, to receive the tongue. But, in this animal, the two thin plates of bone are in the centre; and the parts which surround them are composed of skin and membrane, in which a muscular structure probably is inclosed.

The teeth have no fangs which sink into the jaw, as in most quadrupeds, but are imbedded in the gum; and have only lateral alveolar processes, from the outer and inner edges of the jaw, to secure them in their places, but no transverse ones between the two teeth.

The tongue is extremely short, not half an inch long; and the moveable portion not more than a quarter of an inch; the papillæ on its surface are long, and of a conical form. When the tongue is drawn in, it can be brought entirely into the mouth; and, when extended, can be projected about a quarter of an inch into the beak.

The organ of smell, in this animal, differs, in some particulars, from that of quadrupeds in general, as well as of birds. The external openings of this organ are placed nearly at the end of the beak, there being only the lip beyond them; while the turbinated bones are in the same relative situation to the other parts of the skull as in quadrupeds; by which means, there are two cavities the whole length of the beak, superadded to the organ of smell.

The turbinated bones in each nostril are two in number, and are distinct from each other. That next the beak is the longest, has a more variegated surface than in the duck, and has the long axis in the direction of the nostril; the posterior one is short, projects farther into the nostril, and the ridges are in a transverse direction.

The posterior nostrils do not open directly under the turbinated bones, as in the duck, but about an inch farther back, and are extremely small; the cavities of the nose, in this animal, are therefore uncommonly extensive; they reach from the end of the beak nearly to the occiput.

The beak itself is formed by the projecting bones already mentioned, covered with a smooth black skin, which extends some way beyond the bones, both in front and laterally, forming a moveable lip. This lip is so strong, that, when dried or hardened in spirit, it seems to be rigid; but, when moistened, is very pliant, and, as has been already mentioned, has probably a muscular structure. The under portion of the beak has a lip

equally broad with the upper: this has a serrated edge; but the serræ are confined to the soft part, not extending to the membrane covering the bone, and are not met with in the upper one. The extent of the lips beyond the bones, is distinctly marked in the drawings.

There is a very curious transverse fold of the external black smooth skin, by which the beak is covered, projecting all round, exactly at that part where the beak has its origin. Its apparent use seems to be to prevent the beak being pushed further into the soft mud, in which its prey may lie concealed, than up to this part, which is so broad that it must completely stop its progress.

The nerves that supply the beak, in their general course, size, and number, seem very closely to correspond with those of the bill of the duck.

The cavity of the skull bears a greater general resemblance to that of the duck than of quadrupeds: there is a very uncommon peculiarity in it, which is, that there is a bony falx of some breadth, but no bony tentorium. This is met with in no quadruped that I know of: it is found in a small degree in some birds, as the spoon-bill, and the parrot; but not at all so as to resemble the falx in this animal.

The orifice of the eye lids is uncommonly small, for the size of the animal; but the eye itself was not in a state to be examined.

The external opening of the ear was so small as not readily to be perceived: it is simply an orifice; but the meatus enlarges considerably beyond the side of the opening, and passes some way under the skin before it reaches the organ, which in this specimen had been destroyed. In the duck, the orifice leading to the ear is very large, when compared with the opening in this animal.

When we consider the peculiarities in the structure of the nose of this animal, which lives in water, it is natural to conclude the organ is fitted to smell in water, and the external nostrils are so placed, to enable it to discover its prey by the smell; for that purpose, the animal can apply its nose, with great ease, to the small recesses in which its prey may be concealed.

The structure of the beak is not such as enables it to take a firm hold; but, when the marginal lips are brought together, the animal will have a considerable power of suction, and in that way may draw its prey into its mouth.

#### EXPLANATION OF THE FIGURES.

##### PLATE XXII:

Fig. 1. A view of the beak, to show the situation of the openings of the external nostrils, marked *a a*.

Fig. 2. Another view of the beak, exposing the under portion.

Fig. 3. A lateral view, to show the opening of the lips, and the situation of the eye and ear. *a*. The eye *b*. The ear.

Fig.



Fig. 4. A view of the upper jaw and palate, to shew the teeth in their situation.

Fig. 5. A similar view of the under jaw.

Fig. 6. The bones which form the beak delineated, and the soft surrounding parts only marked in outline.

Fig. 7. A similar view of the bones forming the lower portion of the beak.

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## VII.

*Instances of suspended Animation in Vegetables. By Mr. JOHN GOUGH.*

To MR. NICHOLSON.

S I R,

Kendal, Jan. 20, 1801.

MR. Baker and Sig. Spalanzani have observed and described various aquatic animalculæ which suffer apparent death when deprived of water, but resume their figure and all their natural functions upon being restored; at a future period, to their native element. The latter gentleman has also extended his enquiries, in hopes of finding similar examples in the vegetable kingdom, but with little success; one or two species of tremellæ being all the plants he discovered answering his expectations.

Notwithstanding the great industry of this celebrated naturalist, a plant more perfect in its structure than the gelatinous substances composing the genus tremella, appears to have escaped his notice; which possesses in a high degree the faculty of dying apparently, and reviving again, in order to accommodate itself to the vicissitudes of its situation.

The plant upon which nature, or more properly, the author of nature, has bestowed this remarkable property, is the lemna minor, or common duck's meat. Some remarks which I made accidentally on a pond covered with duck's meat, in the beginning of July, 1797, induced me to imagine that this little plant could hardly preserve its existence during a long fit of dry weather; unless it resembles the wheel animal, in suffering a kind of temporary death when deprived of water; and afterwards experiencing what may be called a resurrection to a second life, upon receiving a fresh supply of its native fluid.

In order to discover in what degree the lemna possesses a power of accommodating itself to the inconveniences of its situation, the following experiments were made. A quantity of the plant was exposed for four or five hours to the sun, in which time it became perfectly dry; a number of leaves which had undergone the preceding process, and appeared dead and withered, were placed, at the end of two days, in a jar of fresh water, where they revived immediately, and continued apparently in a healthy state for three weeks; when

when an end was put to the experiment, from a conviction, that putrefaction would have destroyed the plants in a much shorter time, had the indications of life exhibited by them proved delusive.

The foregoing fact appeared to me, at the time of its discovery, a surprising instance of suspended animation in a vegetable; but a similar trial, made at a subsequent period, afforded a more extraordinary proof of the existence of an accommodating principle in the same plant. A number of leaves, which were dried with those used in the last experiment, had been accidentally preserved in a small box, from the beginning of July, 1797, to the end of March, 1800. Being convinced by trial that the plants were still living, notwithstanding their long continuance in what may be called a torpid state, I placed several of them in a glass jar, which was furnished with a small syphon, for the purpose of changing the water from time to time. The plants, treated in this manner, not only revived after remaining in a state of apparent death for more than two years, but retained sufficient vigour to produce complete parts of fructification in August, which is the proper flowering season of this species of *lemna*.

Various species of *conserva* and *tremella* might be pointed out as endued with the property under consideration; but I shall only mention one circumstance of the kind at present. When ponds dry up in the summer, the sediment left behind forms a paper-like substance on their bottoms: if a piece of this substance be put into water, it turns green in a few minutes, on account of the different *conservæ*, which enter into the composition of this natural paper, and revive upon their being saturated with their proper fluid.

The principle of accommodation that forms the subject of the present essay, is not confined to aquatic vegetables; for in the course of my examination of the subject, I found it in the vegetating germs of the *festuca vivipara*; several of which produced perfect plants after being kept dry some of them four, and others five weeks. The knowledge of this circumstance, and the information I received that barley does not lose the power of sprouting by being malted, determined me to enquire more carefully into the nature of seeds in this respect, and I fixed upon pease for the experiment. With this view I permitted a number of them, which had been moistened for the purpose, to germinate for three days, and then dried them again, by the application of a gentle heat: on soaking them afresh in water, and exposing them to the air, the process of germination recommenced, not by the production of new parts, for the former sprouts revived, and advanced in growth as if no interruption had been previously offered to them. A repetition of the last experiment, made with the same pease, gave a similar result; but a third attempt failed, because the seed lobes parted, their mutual connection being nearly broken before the present trial, by the enlargement of the germs, and the treatment they had received.

The preceding experiments seem to suggest and authorize the following conclusions.

1st. The analogy which has been shewn in many cases to connect vegetables with animals, becomes more extensive the farther we carry our researches; of the truth of which the present essay affords an example.



2d. This analogy appears to be a necessary provision in the instance under consideration, which enables a certain description of organic beings belonging to both kingdoms, to accommodate themselves to the vicissitudes of a situation variable in its nature, and to preserve not only the life of the individual, but perhaps the existence of the species, by submitting to a temporary death, or more properly a complete suspension of animation.

The seeds of land plants, which germinate on the surface of the ground, being exposed to the inconveniences of the weather, in common with aquatic plants growing in shallow ponds, are provided with the same principle of accommodation to insure their safety. The existence of this salutary property is confirmed by experiment; but our knowledge of the nature of vegetables is as yet too limited to discover the singularity of constitution from which it results; we see the wisdom of the contrivance, but cannot investigate the cause.

4th. The peculiarity of constitution, which distinguishes viviparous plants appears to be this: germs of this description begin to sprout when their seed lobes have imbibed a quantity of humidity too small to suffice in common cases; for which reason they frequently vegetate on the stem of the parent plant.

This opinion is corroborated by the following facts:

1st. All seeds germinate when placed in the air, having their lobes saturated with water: on the contrary, they begin to rot when the quantity absorbed by them is too little.

2d. The proportion of water required to produce saturation, in equal weights of different seeds, is different.

3d. Some plants, such as the *polygonium viviparum*, are only viviparous in wet seasons.

I am, SIR,

Your's, &c.

JOHN GOUGH.

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## VIII.

*Extract of a Letter from Doctor G. M. to Dr. William Babington, dated Freiberg, Dec. 17, 1800. On the State of Galvanism and other scientific Pursuits in Germany. Communicated by Dr. BABINGTON.*

MY DEAR DOCTOR,

I HAVE long been waiting for an opportunity of making you some return for the very valuable and interesting intelligence in your last, and now seize the earliest occasion of giving you some account of the galvanic labours in Germany; not but that I think it possible

possible that you have already anticipated them in England: they are, however, of such an important nature; that I think it adviseable to give them to you on chance. The principal galvanic discoverer here is a young man, called Ritter, at Jena, in Saxony: about two years since he published the result of his almost innumerable experiments, in which he established all its laws, and anticipated almost all the newer experiments. Unfortunately the book was written very obscurely, and was still more obscured by the language of the newer philosophy; and the author having neither enemies who were interested in bringing him into discredit, nor friends who were desirous of drawing him out of obscurity, his work remained little known and less noticed; he, therefore, gave up in a great measure the subject, and applied himself to other pursuits, till a few months since the general interest bestowed upon the subject reanimated his zeal. Having satisfied himself that by passing the galvanic influence by means of gold wires through water, the oxygen gas alone was given off at the extremity of the wire connected with the zinc end, and the hydrogen gas from that communicating with the silver end of the pile, he reasoned thus. If each particle of water be composed of a particle of hydrogen and another of oxygen, how can the corresponding gaseous particles first make their appearance at the extremities of the respective wires, placed at one inch distance? can it be supposed that the particle of water is at one and the same time at the extremity of each wire?—this suggested to him to interpose some substance between the extremities of the wires, which was at the same time capable of conducting the galvanic influence, and of remaining perfectly unaltered by it. Such a substance presented itself in highly concentrated sulphuric acid: in a tube of the adjoining figure 6, plate XXI. he poured a portion of the acid, the upper parts he filled carefully with water; in which, by means of cork, he introduced two gold wires within a short distance of the surface of the acid. On suffering the galvanic influence to pass through it, the water in the leg *a* being connected with the zinc afforded oxygen gas alone, while that in the leg *b* communicating with the silver, gave hydrogen gas; and this evolution of gas, as in all other cases, continues so long as the galvanic chain remains uninterrupted. To obviate the objections which might be made, that perhaps one or other of the gases made its way through the acid; he contrived the following experiment. He filled two tubes *a* and *b*, fig. 5, half with acid, and the remainder with water; in each two gold wires were introduced, in such a manner that the extremities of the upper were surrounded with water, while the lower remained immersed in acid, and were connected at *z*. The effect was precisely the same: so long as the tubes stood in the chain, and the wire *a* was connected with the zinc, the water in that tube furnished oxygen gas alone, and the water in *b* hydrogen gas; but no sooner was the manner of connection reversed, than the reverse took place. Lastly, to remove the objection that those two processes are dependant on, and inseparable from, one another, he placed a single tube filled in the above manner in the chain. As long as the wire immersed in water was connected with the zinc, oxygen gas alone presented itself; but when it was turned towards the silver, hydrogen gas; thus it is proved, that water under certain circumstances, may be wholly converted into oxygen gas, and under others, into hydrogen



gas: the rationale of this phenomenon is as yet in obscurity. One philosopher accounts for it thus; that water + light gives oxygen gas, and water + heat hydrogen gas; when these two gases are decomposed, the ponderable part presents itself once more under the form of water, and the fire escapes. Others propose the following; that oxygen gas is water + positive electricity; and hydrogen gas, water + negative electricity. This seems countenanced by numerous observations, among others by some already made in England, viz. that the electrometer is affected negatively on being presented to the zinc extremity of the galvanic battery, and positively at the silver end. I suppose I need not tell you that the atomistic system is almost entirely exploded in Germany, and the dynamic substituted in its stead; consequently, in place of the term *matter*, the phrase *spacefilling* is employed. Thus, according to the newest sect of natural philosophy, water is the only spacefilling in nature that gravitates, and when united to electricity, furnishes the principal gases as seen above; but when to water, magnetism is superinduced, their cohesion is the consequence, and the different solids are the products: the varieties in these result from the predominance of carbon or azote; the former being water + the north pole of the magnet, and the latter water + the south pole. Water itself is perfect indifference, being neither electric nor magnetic, yet the fruitful parent of all!—This theory, certainly very simple, and possibly very sublime, will probably appear as extraordinary to you, who are sunk in the quagmire of matter, as it did to me. You are probably well acquainted with the progress the Parisian chemists have made in the decomposition of the alkalis and alkaline earths: their compound nature has ever been the opinion of Mr. Werner, from the incontrovertible facts which geognostic observations furnish; there can also be no doubt but the flint, argil, &c. are equally compounded. I have lately repeated Guyton's experiments with the alkaline solutions of flint and argil: when these are well concentrated and mixed in equal proportion, a firm, gelatinous, opalescent, mass results in a very few minutes; this is perfectly insoluble in water, yet soluble in acids, whether concentrated or diluted, nay, even in distilled vinegar, and yet consists of both flint and argil: here, therefore, the properties of the flint must be considerably altered. This must render all analysis with alkalis suspicious, and shows on what fallacious grounds the proud dominion of chemistry rests, which she has exercised so long in such an arbitrary and overbearing manner in the mineral kingdom. \* \* \* \*

## IX.

*On the Chemical Effects of the Pile of Volta. By a Correspondent.*

## LETTER II.

TO MR. NICHOLSON.

SIR,

I HOPE I shall not trespass too much on your time by insisting a little further on the difficulty, which I took the liberty of submitting to you respecting the decomposition of water by the Pile of Volta, and which you honoured by inserting in your truly valuable Journal. I am not at all disposed to believe that chemical effects, dependant on each other, should always take place close to each other; although I contend that we should be able to ascertain the cause which produces a deviation from local proximity.

That some distance of time intervenes between chemical effects, some experiments, in which I have been engaged for a considerable time, have fully convinced me. I have long thought, that as Nature produces some of her most curious and perfect combinations by very slow and regular processes, that as she destroys her finest productions when she operates at high temperatures in volcanic countries, where every thing alike assumes the forms of cinders, ashes and lava, that the usual modes of operating chemically are defective in as far as we wish, by the application of great degrees of heat and sudden strong attractions by mixture, to produce immediate results in new combinations and new substances. I have succeeded in reversing many acknowledged attractions, by only giving time for the proper arrangement of the particles. I shall mention one decomposition which requires a very considerable time for its production. Why, if the attractions tend one way, they do not instantly arrange themselves, I cannot at present determine. Having found that a solution of sulphuret of pot-ash attracts oxygen from the atmosphere, I thought it might be employed for the decomposition of the carbonic acid. With this view, I inclosed in a bottle a quantity of carbonate of lime, and a strong solution of sulphuret of pot-ash. I shook them well together, but I did not observe any change after a considerable time. The bottle, however, remained in my room closely stopped for six months, when I observed that the sides of it were covered as high as the solution reached with a black powder, which had the properties of charcoal. I could produce many other instances to prove that combinations will take place, as it were spontaneously, if time be allowed for proper arrangement. Organized substances in particular afford very striking instances. Phosphorus, though so strongly attached to oxygen in animal substances, is nevertheless frequently separated by a slow decomposition.

In the experiment of the vegetations of a metal by means of zinc, which you produce as a parallel fact to the decomposition of water from the pile, I find some modes of reasoning which



which seem sufficient to account for the phenomenon. The laws of crystallization will apply to explain why the lead assumes a regular figure, as other metals; for instance tin, assume their own forms. You may also have remarked that, when a metal is dissolving in an acid, the solution descends through the solvent (as happens in the oxidation of copper by wires of that metal in water, and connected with the pile) according to the increased specific gravity of the newly formed compound. It is also known, that there are different degrees of saturation between metals and acids, which may take place as the heavier fluid descends; for you will observe that the vegetations generally take place downwards. I have observed that, when zinc is placed in a saturated solution of lead or tin, a very confused mass of precipitated metal forms very soon around it, and that, in order to produce fine crystallizations, it is necessary to add a large proportion of water: this seems to shew that the descent of the new metallic solution has some share in producing the effect. But in these experiments continuity is preserved, and the same effects are produced in every crystallization of any salts.

It appears to me therefore that the separation of the two gases from the connection with the pile of Volta, is an anomalous effect in chemistry, as yet unexplained according to the doctrine of the decomposition of water. It has been a long time known that positive and negative electricity have different properties; it is now clearly shewn that they produce different chemical effects. I do not see how, in strict philosophy, we are warranted in saying more of the experiment with the pile, than that one kind of electricity and water produce one kind of gas, and the other another gas. Is it not an assumption in this experiment to say, that the bases of the gases are the component parts of water? I could start other difficulties even where there appears to be a clearer proof of the decomposition of water, but, fearful lest I should have already too much imposed upon your kindness, I hasten to subscribe myself,

Your obedient humble Servant,

AN EXPERIMENTALIST.

January 20, 1801.

X.

*Analysis of the Mellite, or in German, Honigstein. By CIT. VAUQUELIN\*.*

THE analyses which Messrs. Abich and Lampadius have given of this interesting fossil are well known. The first obtained from one hundred parts, 16 carbonate of alumine, 4 carbone, 3 oxyde of iron, 40 carbonic acid, 28 water of crystallization, having a smell of bitter almonds, and 5.5 of Naptha.

\* Annales de Chimie XXXVI. 203.

The second had for his result 86.4 of charcoal, 3.5 petroleum, 2 flax, and 3 of water of crystallization. An enormous difference\*.

Mr. Abich proposes on account of the incombustibility of the honigstein, that it should be removed from the class of combustibles, and placed in that of the aluminés.

But Mr. Klaproth, whose labours are intitled to the highest confidence, wrote me several months ago, that he has discovered that this pretended stone is composed of a peculiar vegetable acid united to alumine.

M. Abildgaard, to whom I am indebted for many interesting minerals of Norway, sent me some decades ago, by favor of M. Mantey, a small quantity of honigstein, of which he had designed part for analysis: and I hastened to comply with his wish in that respect.

#### *Description of the Honigstein.*

This substance has a light yellow colour, which has caused it to obtain the name of mellite, or honey stone. It is usually crystallized in an octahedron, of which the angles are sometimes replaced by faces, owing to laws of diminution, of which the laws have not yet been limited.

Its specific gravity is not considerable. It is according to M. Abich 1.666. It is found in Thuringia in strata of fossil bituminous wood.

#### *I. Chemical Character of Honigstein.*

When exposed to fire with the contact of air it becomes white, and burns without becoming sensibly charred; it leaves a white matter, which produces a slight effervescence with acids. It has no sensible taste; though if it be kept for some time on the tongue, it occasions a slight impression of acidity.

#### II. ANALYSIS.

I took two grammes of the honigstein reduced to powder, and mixed them with four grammes of saturated carbonate of pot-ash dissolved in a sufficient quantity of water. As soon as the mixture was made, an effervescence of considerable activity followed, without the assistance of foreign heat; but to accelerate, and more effectually to complete the decomposition of this substance, I slightly heated it on a sand bath.

The filtered liquor after cooling was of a brownish colour, and left on the filtering paper a brown matter, which when dried in the sun weighed very nearly 0.8 of a gramme.

\* If M. Lampadius operated upon the same substance as Mr. Abich and myself have analyzed, it is impossible he could have had 86.4 of carbone; for in forty parts of carbonic acid, and four of carbone, obtained by Mr. Abich, there is not the matter to form eighty-six of carbone: and as from my analysis it appears to me that there is not more than 55 per cent. of real acid in the honigstein, it is evident that it cannot afford eighty-six of charcoal. It must therefore necessarily follow, either that Mr. Lampadius did operate upon another substance, or that he did not heat it sufficiently to decompose the acid, if he did in fact operate upon the honigstein.

This



## III.

This 0.8 of a gramme of brown matter calcined in a crucible became white, and then weighed only 0.33 gramme. When mixed with diluted sulphuric acid, it produced a slight effervescence. The mixture was afterwards evaporated to dryness.

I expected from the information of Mr. Klaproth, that by the addition of water, nearly the whole of this matter would have been dissolved; but the contrary happened, for the greatest part remained in the form of a white powder.

The fluid being evaporated to the point, at which there remained at most not more than three or four grammes, I added one drop of sulphate of pot-ash, and obtained, by leaving it to spontaneous evaporation, about 0.1 gramme of alum, mixed with a small quantity of sulphate of lime.

I afterwards examined what might be the nature of the substance, which after the treatment with sulphuric acid remained insoluble in water.

For this purpose I boiled it in a solution of carbonate of pot-ash; then filtered, washed, and examined it as follows:

1. The muriatic acid, diluted with two parts of water, attacked it with a slight effervescence; but the solution did not become clear; on the contrary it remained somewhat milky.

2. This filtered liquor afforded with ammonia a precipitate resembling in its transparency that which alumine affords by the same means; but it was not entirely soluble in pot-ash.

But the portion dissolved by the pot-ash was the most abundant, and presented all the characters of alumine: for when combined with the sulphuric acid it afforded alum. The cause why this portion did not remain combined with the sulphuric acid was therefore, most probably, that it had been too much heated towards the end of the desiccation.

The fluid from which the ammonia had separated the alumine last mentioned, afforded also slight precipitates by the carbonate of pot-ash, and the oxalate of ammonia; which proves that it contained a small portion of lime.

The portion of matter which was not dissolved by the pot-ash, weighed at most 0.1 gramme, and appeared to me to be flex.

The honigstein therefore contains a small portion of lime and of flex.

After having ascertained the matter which composed my residue, I directed my operations to the liquid which must have contained the acid of mellite combined with pot-ash; and with the expectation that it would yield its basis to the mineral acids, I added to a portion of the liquid some drops of nitric acid, which produced a very slight effervescence, and disengaged a very small portion of brown flocculent matter. Some hours afterwards, that which I suspected came to pass; the acid of the mellite crystallized in the form of small short prisms with brilliant faces.

Finding that this method would succeed for separating the whole of this acid from the pot-ash, I slightly heated the whole of the fluid, and added nitric acid till there was an excess perceptible to the taste. I then filtered to separate the brown flocculent matter, and obtain the acid in a state of greater purity; and in effect I obtained about 1.34 grammes, which was white enough, though still of a yellow tinge. The following are the properties which presented themselves on its mixture with different substances.

1. This acid has brilliant facets, considerable hardness, slight acid taste, accompanied with bitterness, which may arise from some portions of bitumen still attached to it, and to which it owes its yellow colour.

2. A portion of this acid exposed to the flame of the blow pipe, gave at first a few scintillations like those of salt-petre; after which it swelled up, and left a substance that soon penetrated the charcoal.

3. When heated in a covered crucible of platina it swelled up at first, afterwards became charred, without affording an oily flame, and left a light coal which is very alkaline\*. This acid was therefore combined with a certain quantity of the pot-ash, notwithstanding the excess of nitric acid which had been added to its solution. The same effect also takes place with the tartareous and oxalic acids, which by similar treatment are made to pass to the state of acidules.

4. This acid is sparingly soluble; but I have not exactly determined the quantity of water which it requires.

5. Several grammes of the same acid, dissolved in water, were mixed, 1. with a solution of lime: in which a white flocculent precipitate was immediately formed, that soon fell to the bottom of the fluid. 2. With a solution of sulphate of lime, in which a light granulated and crystallized precipitate was found, that left the water still in a certain degree transparent; but which was increased and rendered flocculent by the addition of one drop of ammonia†. 3. With the solution of muriate of barytes it affords little precipitate the first moment, but soon afterwards a shower of crystals in needles. 4. With the solution of silver it gives a white silky precipitate, which shines like a solution of soap: and sometime afterwards it subsides in the form of powder. 5. With the nitric solution of lead it affords a white pulverulent very heavy precipitate. 6. With that of mercury a white precipitate, which a single drop of ammonia turns black.

\* This acid must not be confounded with the acidulous tartrate of pot-ash by this property. For the latter swells up much more, and emits at the time of its decomposition a dense fume, of a peculiar and very distinguishable odor.

† The acidulous tartrate of pot-ash does not immediately produce a precipitate in the solution of sulphate of lime; but twenty-four hours afterwards there is found in the mixture crystals with very brilliant facets, composed of lime and tartareous acid. Though crystallized, this tartrate of lime does not resemble the combination formed with the acid of honigstein, and the same base. It differs from it in swelling up in the fire, whereas that is decomposed without swelling, in which respect it has analogy with the oxalate of lime.

From



From the result of these experiments we see, that the acid of the honigstein has many properties analogous to those of the acid of sorrel, and by the comparison I have made, I have perceived no difference but the following: 1. The precipitate which it causes in the solution of sulphate of lime is less speedily manifested, and is crystalline, instead of being pulverulent, like that which is formed by the acidulous oxalate of pot-ash. 2. It appears less acid to the taste than the acidulous oxalate of pot-ash; but this may depend upon my not having added enough of nitric acid to the combination with pot-ash, to deprive it of a sufficient quantity of the alkali; and 3. It swells up rather more by heat than the acidulous oxalate of pot-ash.

In other respects the sublimed salt, the great quantity of carbonic acid, and the little coal which the mellite affords by distillation, are facts that seem to unite in proving the identity of the two acids: for the habitude of the salt of sorrel by fire is the same\*.

The octahedral figure of the honigstein appears also to have analogy with that of the oxalic acid, which is a rectangular prism terminated by pyramids with four faces. It is only necessary to examine the inclination of these faces in order to obtain a certainty.

Nevertheless, as I have had no more than about 1.34 gramme of this acid at my disposal, I have not been able to subject it to all the trials which would have been required to demonstrate in the most absolute manner its identity in all respects with the oxalic acid. For though they have presented similar phenomena in all the comparative trials I have made, it is possible that by other that might yet be made, there might be found a single difference which would destroy the similitude.

It is therefore chiefly with a view to engage the chemists of Germany, where this substance is more common, to recommence the analysis of honigstein, and to compare its acid in every possible respect with those of sorrel that I publish this notice.

If my opinion should be confirmed by new experiments, we shall then possess the oxalic acid in all the three kingdoms; namely, in the state of acidulous oxalate of pot-ash in several kinds of vegetables; in that of oxalate of lime in the human urinary calculi; and lastly, in the state of oxalate of alumine in the bowels of the earth among bituminous wood. But in whatever part it is found, it appears to owe its origin to vegetable substances.

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*Note,* Since the above notice was drawn up, I have thought of mixing the acid of honigstein combined with a small quantity of pot-ash, with a solution of pure sulphate of alumine, and a very abundant flocculent precipitate was immediately afforded. On the

\* The acid of sorrel, or oxalic acid, is that which affords by distillation the largest proportion of carbonic acid and water, because it contains more oxygen than any other known vegetable acid.

other hand, I put into the solution of the same salt the acidulous oxalate of pot-ash; but no precipitation was seen. Now these different effects must afford doubts sufficiently strong respecting the identity of the acid of honigstein with that of sorrel: and I must confess, that I shall suspend my judgment in this matter, notwithstanding the disposition I had at first to believe, that these two acids are of the same nature. Before we decide, we must therefore wait for new proofs derived from a larger scale of experiments, in order to throw a more decisive light on the subject.

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Cit. Haüy has carefully examined this substance. Some crystals of it were presented to him by M. Abildgaard, and he procured some others from the collection made in Germany by Cit. Launoy. He has discovered, that the mellite has a very obvious double refraction, whence there arises a distinct character between this mineral and amber, of which the refraction is simple. He has observed likewise, that the crystals of mellite being insulated, acquire with much facility the resinous electricity, but he was not able when they were not insulated, to excite in some of them more than a weak and varying electricity; so that it was necessary to bring quickly together the crystal and small copper needle that is used in these kind of experiments, in order that the latter may be more sensibly attracted. What several learned men have advanced, that this mellite is not rendered electrical by friction, is not exactly true: it may then acquire an electricity of the same nature, as that of amber, but it would be considerably weaker if the crystal be not insulated.

According to the observation of the same naturalist, the primitive form of the mellite determined according to the position of the natural junctures, is that of a rectangular octahedron, in which the incidence of the face of one pyramid with the face of the other, is about  $93\frac{1}{2}^{\circ}$ . This octahedron is sometimes truncated by means of a decrease, by an arrangement on all its solid angles, in which case the two facets which are placed on its summits are often curve lined. When that the diminution only acts on the four lateral angles a dodecahedron is produced, which considerably resembles the rhomboidal figure. Supposing this resemblance perfect, this would be a fourth origin of that dodecahedron, which exists as primitive in granite and sulphurated zinc, and has in other substances, where it becomes a secondary form, sometimes as nucleus a cube, and at others a regular octahedron.

*Bulletin des Sciences, No. 43. An. 9.*



## SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &amp;c.

*Method of ascertaining the Inclination of the Magnetic Needle. By CIT. COULOMB.*

**T**HOUGH the instrument which is usually employed to measure the inclination of the magnetic needle is very simple in its construction, it is nevertheless liable to great errors, which in general arise from the almost absolute impossibility of placing the needle in all the positions it can take in equilibrium with regard to the effect of gravitation, that is to say, so that its centre of gravity may always exactly agree with the point on which it turns. When the dimensions are considerable, a new inconvenience arises from a degree of flexure which, though scarcely sensible, is nevertheless productive of very great effects from the slightest displacement of the centre of gravity producing a combination of the power of gravitation with that of magnetism.

To obviate these difficulties, Cit. Coulomb, instead of endeavouring to ascertain immediately, as has been hitherto done, the direction of the magnetic needle in the vertical plane which passes through the magnetic pole, conceives the force of this pole to be decomposed or resolved into two others in the same plane, the one acting in a horizontal, and the other in a vertical direction. He determines separately the intensity of each of these last forces, and the result gives the direction in which the magnetic force acts, and which a needle governed singly by this force would take.

Cit. Coulomb has proved, in the Memoirs of the Academy of Sciences for the year 1789, that the magnetic needle suspended by its centre of gravity is incessantly brought back to its true direction by a constant force at the same place and time. It thence follows that by observing the number of oscillations made in a given time by a needle horizontally suspended, the ratio of the horizontal component part of the magnetic power with gravity may be obtained. As to the vertical component part, it is measured by determining with care the weight necessary to be added to the southern part of the magnetic needle, to maintain it in a perfectly horizontal position. That being done, if A and B represent the respective measures of the horizontal and vertical component parts of the magnetic power  $\frac{B}{A}$  will be the tangent of the angle made by their result with the horizontal force, and, consequently, it will be the inclination of the magnetic needle.

In the experiments made by Cit. Coulomb the needle had the form of a right angled parallelipipedon; very thin in proportion to its breadth, and always suspended, so that its breadth was kept in a vertical plane. Let P represent the weight of the needle,  $l$  the half of its length,  $\lambda$  the length of a pendulum that performs its oscillations in the same time as the needle when it obeys the magnetic power in an horizontal plane. Cit. Coulomb then

gives the formulæ  $\frac{P l^2}{3 \lambda}$  to calculate the momentum of the magnetic force referred to the arm of a lever of one millimeter in length. The length of the needle was 427 millimetres, its breadth 13, and its weight 88753 milligrammes. It was suspended horizontally by a thread of silk in a box well closed, and it made 30 oscillations in 286 seconds, and by applying these data to the preceding formula, Cit. Coulomb finds that the logarithm of the momentum of the horizontal magnetic force is 4,1740.

Cit. Coulomb having placed his needle in a clip, having knife edges, which rested on two cylinders of glass, in the manner of the beam of a balance, endeavoured first to bring it to an equilibrium in an horizontal situation coinciding with the magnetic meridian, by placing the edges in a proper manner, and when they were sufficiently near the point where the equilibrium took place, he completed it by the addition of small weights. He then reversed the poles of the needle by the magnetic touch, but without altering the position of the clip, and again bringing it to an equilibrium in this new state, the sum of the momenta of the additional weights placed in these two operations gave him the double of the momentum of the vertical component parts of the magnetic force, valued at  $\frac{74467}{2}$ . The result of this force, and of the horizontal force, is inclined  $68^\circ 9'$ .

In repeating these operations three times, Cit. Coulomb obtained successively  $68^\circ 9'$ ;  $68^\circ 13'$ ; and  $68^\circ 11'$ . Though the differences of these results are very trifling, he does think they are to be entirely attributed to errors in the observation; for he is assured that they do not amount to so much. It is possible that the needle is subject to variations in the vertical similar to those which are known to take place in the horizontal plane.

*Bulletin de la Soc. Phil. No. 31. An. 8.*

#### *New Production of Ammonia.*

Mr. Lampadius, at Freyberg, has observed that when crude, or which is better, purified acidulous tartrate of pot-ash, is heated till no more fumes or flame appear, and water is then added to it, ammoniac is produced. This fact is best observed when heated mass is still warm (about 120 R.)

The experiment may be performed with the same tartrate repeatedly, or at least as long as any carbonaceous matter remain, by merely heating it, and then wetting it with a few drops of water. Carbon mechanically joined to pot-ash does not produce this effect; but the acidulous oxalate of pot-ash, treated in the same manner, affords a similar result.

*Crell's Annals of Chemistry, No. 8, 1806.*

Upon



Upon trial of the above before some philosophical gentleman at my house, the fumes which arose upon the addition of the water did not indeed smell of ammonia, but they formed a white cloud round the stopper of a bottle which was wetted with strong muriatic acid. The indication was not so manifest as to remove all doubts in the minds of the assistants; but Mr. Accum has since informed me that he has found it much stronger and more constant when paper, wetted with muriatic acid, was used.

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*Discovery of a new Alkali.*

Dr. Hahneman \*, at Altona, in Germany, has discovered a new species of alkali, which he distinguishes from the others by the name of *pneum-alkali*, on account of the singular property it possesses, of increasing in bulk to twenty times its size when heated. This alkali differs greatly from any of the others, by the following properties.

Its crystals are hexaedral prisms, terminated by two inclined faces, one of which appears to be trihedral, the other pentahedral:

It is not volatile by heat.

It neither attracts moisture, nor effloresces by exposure to the air.

Two parts of distilled water dissolve one of this alkali at  $+ 300^{\circ}$  F. Five hundred parts of water dissolve one hundred and forty parts at  $60^{\circ}$  F. At a lower temperature water acts upon it very slightly: it is, therefore, separated from its former solution by mere cold: it may thus likewise be separated when combined with any of the other alkalies.

It effervesces but slightly with concentrated acids; but it is not yet determined if the disengaged aeriform fluid is carbonic acid, or any other gaseous fluid.

The sulphate of *pneum-alkali* is totally insoluble in ardent spirit. Water dissolves only a small quantity.

The nitric, muriatic, phosphoric, and acetous acid, readily combine with it, and the neutral salts resulting from these combinations are totally soluble in water and ardent spirit. The phosphate and acetite of *pneum-alkali* are in considerable quantity even soluble by cold in spirit of wine.

The phosphate of *pneum-alkali* has a bitter taste.

The muriate crystallises in plumose crystals.

All the preceding combinations of this alkali with acids (the phosphate excepted) are decomposable by mere heat alone. They all part with their acids, and leave the *pneum-alkali* behind in a pure state. The inventor of this alkali employed for this purpose a red heat. If a less heat will do, I do not know.

\* Scherer's Chem. Journal, III.

† Quere.

The nitrate of pneum-alkali liquifies in its own water of crystallization at  $300^{\circ}$  Fr. It first bubbles much, nitrogen gas is evolved, and the salt is long decomposed before the mass is ignited. The nitrate of pneum-alkali does not detonate on ignited coal, it does not decrepitate, nor does it become phosphorescent, nor does it emit sparks. Its decomposition by heat is affected gradually and quietly.

The union of pneum-alkali with carbonic acid is extremely difficult. It is best effected by decomposing a saline combination of this alkali with an excess of acid, by means of any of the other carbonated alkalies: the first precipitate which is obtained by this means, is the *pure pneum-alkali* in a crystalline form; but soon afterwards the carbonate appears in the state of a light, white, pulverulent, earthy substance. The carbonate of pneum-alkali readily parts with its carbonic acid at the common temperature of our climate, and becomes converted into pure pneum-alkali.

It separates earths and metallic substances from their saline combinations.

Mercury, when agitated with the aqueous solution of this alkali, does not lose its lustre.

It precipitates the oxygenated muriate of quicksilver of a carmine red, the nitrate of mercury black, the nitrate of silver white, &c. &c.

It decomposes muriate of ammoniac at  $100^{\circ}$  F; but water, impregnated with ammoniac gas, separates the pneum-alkali from the neutral combinations by mere cold.

A saturated solution of this alkali unites with fat oils by mere agitation, and forms a saponaceous compound, soluble in spirit and in water. Ardent spirit has no effect upon it. It changes blue vegetable tinctures green, and possesses some of the properties common to the other alkalies, but again many others, widely different, which entitle it to the notice of the philosophical chemist.

This alkali is sold at C. G. Hilscher in Leipzig, in bottles containing one ounce, at one Frederic d'or.

### *Flour from the Bread Fruit.*

Citizen Van Noorden, physician at Rotterdam, has written to the (French) Society of Arts, that a surgeon, who lately arrived from Surinam, informs him, that the bread fruit tree has succeeded so well in that colony, that there are considerable plantations, which are productive beyond all expectation. They make bread from it, which is as good as that afforded by wheat. For this purpose the fruit is sliced, then dried in the sun, and afterwards pounded; and the flour, when made into paste with water, rises as well as the flour of wheat, and may be kept for a considerable time. The expectation which has



justly been entertained of introducing this valuable tree into the western colonies, and even into Europe, will give additional interest to this account of the new application of the bread fruit at Surinam.

*Bulletin de la Société Philom. No. 44, An. 9.*

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*Extract from a Memoir of Cit. Thénard, on the several Degrees of Oxigenation of the Oxide of Antimony, and on its Combinations with sulphurated Hydrogen.*

Cit. Thénard divides his memoir into four paragraphs.

In the first he relates the principal experiments that have been made since Geoffroy to the present time.

In the second he treats of the several oxides of antimony, and shews that this metal is capable of being combined at least in six different proportions with oxygen; that when oxidized to the minimum, it is first black, then chestnut brown, orange, yellow, and white; and at the maximum also white;—that diaphoretic antimony is a combination of this last with pot-ash, and not a pure oxide, as has hitherto been imagined: that the second, white oxide which is least oxidized, comprehends the sublimed oxide of antimony, that which enters with the composition of the emetic tartar, and also of the butter of antimony, which, consequently, ought to be erased from the list of oxigenated muriates, where it is placed; that all these oxides, when heated in a well closed crucible, are reduced, and the more easily the less they are oxidized; and thus produce the oxides of a yellow, orange, and chestnut brown, and the black oxide, which is also obtained with more facility by precipitating the solutions of antimony by iron, and possesses the remarkable property of being pyrophoric.

In the third part, the author gives the analysis of kermes and the golden sulphur, and shews that the alterations to which these bodies are subject from the action of air and light are owing to the decomposition of these fluids: that in the kermes the oxide is in the state of the brown oxide, and in the golden sulphur in the state of the orange oxide; that the cause of different colours of the kermes, which are obtained, arises from the different coloured oxides which these kermes respectively contain. He afterwards gives analyses of the sulphuric acid, the sulphate of barytes, and of sulphurated hydrogen, together with the specific gravity of this last, and then proceeds to shew the action of the alkaline bases on the sulphuret of antimony, and shews that the kermes is held in solution by the sulphurated hydro-sulphuret of the base, formed by the decomposition of water; that accordingly as this sulphurated hydro-sulphuret has, or has not, the property of being more soluble with heat than without, the deposition by cooling is found to take place on the contrary; and, lastly,

lastly, that the liquor when cold throws down, by means of the acids, the golden tartar, and not kermes, because the brown oxide becomes more oxidized by the oxygen of the water and passes to the state of the orange coloured oxide.

In the third paragraph Cit. Thénard makes a recapitulation of his experiments.

*Bulletin des Sciences, An. 8. No. 31.*

*On the Structure of the upper Pyreneans, by CIT. RAYMOND. Communicated to the National Institute of France.*

Pallas in Asia, Sauffure, Deluc, and Dolomieu in Europe, have observed, that in large chains of mountains, there is usually in the center a more elevated chain of granite, accompanied on each side by a collateral schistus chain, and still lower by another, which is calcareous.

The Pyreneans appear to accord with this law. Their highest points are certainly calcareous, and this circumstance has embarrassed observers.

Cit. Raymond has ascertained, that the respective disposition of the five orders of mountains, exists no less in these than in the other chains, but with this difference, that the calcareous chain on the Spanish side is the most elevated of the five, and that in returning on the side towards France the southern schistus chain is found, then the granite, or middle, and the northern schistus and calcareous chains, gradually diminishing in height, which is the cause why the geological axis of the Pyrenees, or the granite, is not the same as the geographical axis, or that which determines the course of the waters.

In order to demonstrate the accuracy of his observations he has drawn on the chart five lines, corresponding with the five orders of the mountains, each of which is found to pass through the summits or masses of the substance, which forms the character of that order which the line indicates.

The granitic axis passes through the summits of Néouvielle, Pic long, Bergons, and Monné, the schistose chain, and the northern Gneiss, through the Pic du Midi, and the southern through those of Troumouze, Pic mené, Vignemâle, and the Pic du Midi de Pau.

The calcareous ridges on the side of France are those of Campan and of Sarrancolin, so much celebrated for the marble they produce, and those on the side of Spain form Mont Perdu, Marboré, and le Pic blanc, which are among the most elevated summits of these mountains.

*Abridged in the Bulletin de la Soc. Philom. No. 41, An. 3.*



*Letter from Mr. DAVY, Superintendant of the Pneumatic Institution, containing Notices concerning Galvanism.*

TO MR. NICHOLSON.

S I R,

In pursuing my enquiries concerning the production of galvanic influence during metallic oxidation, I have found that many of the difficultly oxidable metals may be made to act as Voltean combinations, by being connected in pairs in the common order with fluids capable of oxydating one of the alternate metals.

Ten silver plates attached to thin gold wires, and arranged in glasses containing diluted nitric acid, produced when their agency was applied in the usual mode, a strong caustic sensation on the tongue, and effected, though feebly, the usual changes in water.

Twenty pieces of copper in contact with silver wires, when connected with weak solutions of nitrate of mercury acted powerfully; and that for a great length of time, *i. e.* till almost all the mercury was precipitated on the copper. The influence produced sensible shocks. When it was passed through water by means of gold wires, oxygen was given out at the place of the copper, and hydrogen at the place of the silver. Whereas in the combination with silver and gold, the oxygen was produced at the place of the silver, and the hydrogen at that of the gold.

The agency of galvanism upon inorganic bodies appears to be similar under all its different modes of excitement. I have found that the gases evolved from water by the action of series in which the oxidating fluid media are acids, or metallic solutions, do not differ in kind or properties from those produced by that of combinations in which the fluid media are constituted by common water.

I have lately made many experiments on the single oxidating circles of Ash, and on the influence of those circles on galvanic animal irritation. These experiments will at some time be made public; they go far towards proving not only that the circles of Ash are governed by the same laws as the pile of Volta; but likewise that there exists in living matter galvanic action independent of all influence generated by metallic oxidation. I have produced the phenomena of taste and muscular irritation by means of metals, in cases when they were apparently incapable of undergoing chemical change.

I am, SIR,

with respect your's,

Dowry Square, Hotwells,  
Jan. 23, 1801.

HUMPHRY DAVY.

*A new*

A new edition of Parkinson's Chemical Pocket Book, with improvements, is on the point of being published.

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Mr. William Henry, of Manchester, has in the press, and in considerable forwardness, a small work, intended, partly, to facilitate the acquirement of chemical knowledge to persons entering on the study, without the aid of an instructor; and, partly, as a portable companion, for the use of more advanced students. The first part will contain directions respecting the best mode of studying chemistry; and, also, an arranged series of experiments, necessary to be performed, by those who intend to become acquainted, by actual observation, with the chemical properties and habitudes of bodies. More minute directions will be given for conducting these experiments with success, than are to be found in other elementary books. The second part will comprize summary instructions for the analysis of mineral waters, and of mineral bodies in general: and the third part will point out some of the useful applications of chemical agents, in detecting adulterations, in discovering poisons, &c. The work will form one small pocket volume; and the author thinks proper to observe, that it will not at all interfere with the excellent little manual, lately published by Mr. Parkinson, the plan and objects of which are perfectly different.

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\* \* J. W. L. requests the Correspondents of this Journal to point out *a method of obtaining aluminous earth perfectly free from any heterogeneous mixture*, and observes, that the methods pointed out by systematical writers are insufficient for that purpose. As this difficulty will no doubt be new to many chemists, it would be a desirable communication if he were to state the imperfections which his practice may have led him to detect.



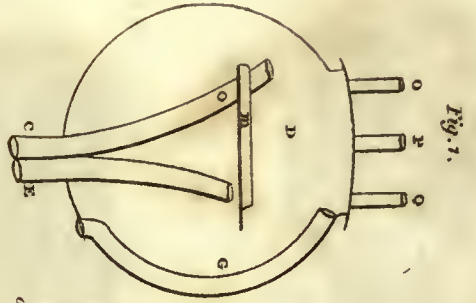


Fig. 1.

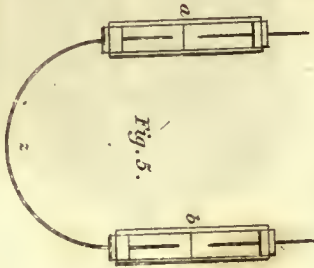


Fig. 5.

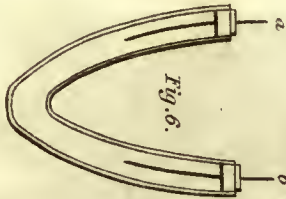


Fig. 6.

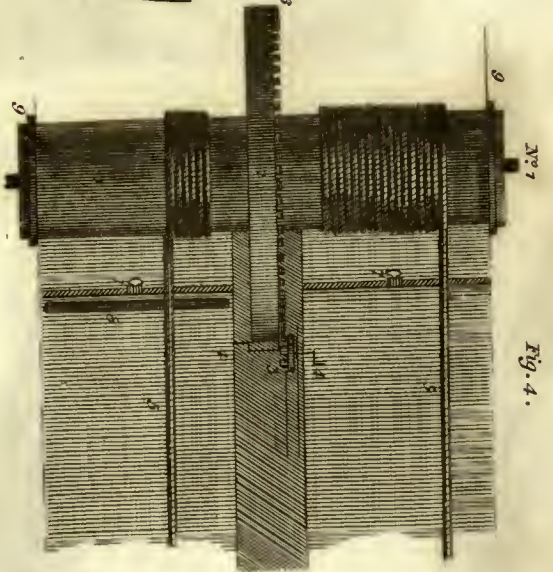


Fig. 4.

*Galvanism*

Surface of the Water in the higher Level

Horizontal Line of the upper Level

Fig. 2.

Line of Indication of the underground Plane

Surface of the Water at the lower Level

Fig. 3.

GROUND PLAN

He the rough Glass





Fig. 1.



Fig. 2.

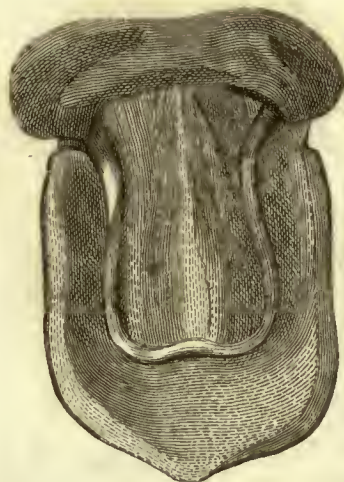


Fig. 4.



Fig. 3.



Fig. 6.



Fig. 5.



Fig. 7.







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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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MARCH 1801.

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ARTICLE I.

*Experiments to determine whether or not Fluids be Conductors of Caloric. By THOMAS THOMSON, M. D. Lecturer on Chemistry in Edinburgh. Communicated by the Author.*

IT is rather surprising that the experiments published by Count Rumford, in order to prove that fluids are non-conductors of caloric, should have been in the possession of men of science for more than two years, without any attempt either to confirm or refute them. These experiments were conducted with such admirable simplicity and ingenuity, by a man of such acknowledged talents and candour, and of such well-merited celebrity, that they might have been expected to excite peculiar attention. The subject, too, is of the highest importance; not only because it is calculated to throw additional light on the nature of caloric, but because it is intimately connected with some of the most important operations in nature. It is impossible for us to investigate spontaneous evaporation, rain, winds, the changes of temperature in the atmosphere, and some of the most important phenomena of chemistry, at least with any chance of success, till we have settled the previous question, whether fluids be conductors of caloric or not. Perhaps it will be said, that this point has been completely established by Count Rumford. This is the very thing which I propose

pose to examine. But it will be proper, in the first place, in order to prevent verbal disputes, to explain what I mean by the *conducting power* of bodies. Count Rumford has neglected to do this; and I am not absolutely certain that he affixes the same meaning to the phrase that I do.

Dr. Herschel appears to me to have demonstrated, that caloric is emitted from the sun in rays, and that it moves with the same velocity as light, or at the rate of nearly 200,000 miles in a second. It follows, from his experiments, and from those of Scheele and Piccet, that caloric moves through air without any sensible diminution of its velocity. It has been demonstrated, too, that it is capable of moving through glass of a certain temperature, without any sensible diminution of its celerity. Now, whenever caloric passes through a body with undiminished celerity, we may say that it is *transmitted* through the body. *Air*, then, and *glass*, have the property of *transmitting* caloric through them. When caloric is transmitted through a body, as its velocity is too great to be measured, it appears to pass through the body instantaneously. Thus M. Piccet found that caloric moved instantaneously through 69 feet of air.

When the end of an iron rod, 20 inches long, is put into the fire, while a thermometer is applied to the other extremity, it is four minutes before the thermometer begins to rise, and it takes 15 minutes before it rises  $10^{\circ}$ . Caloric, then, does not pass through a rod of iron instantaneously; consequently, it is not *transmitted* through the iron, but moves through it in a different manner. Its velocity is prodigiously diminished, since instead of moving at the rate of 200,000 miles in a second, it moves only at the rate of 20 inches in 4'. Now, whenever caloric passes in this manner through bodies, with its velocity prodigiously diminished, it is said to be *conducted* through them. It appears, then, that caloric is *transmitted* through bodies, and *conducted* through them, in quite a different manner.

If we take an iron rod, and another of baked clay, of precisely the same size and shape, and, putting one end of each into the fire, apply thermometers to the other extremity, we shall find that the thermometer applied to the iron rod rises sooner than that applied to the clay rod. Caloric is then conducted with more rapidity through iron than through baked clay. Consequently, iron is a *better conductor* of caloric than baked clay. When a body does not allow caloric to pass through it at all, it is called a *non-conductor*.

It is not difficult to form some notion of the manner in which caloric is conducted through bodies. As its motion is prodigiously retarded, it is clear that it does not move without restraint. It must be detained for some time by the particles of the conducting body, and, consequently, must be attracted by them; hence it follows, that there is an *attraction* or *affinity* between caloric and every conductor of caloric. Now it is in consequence of this affinity that the caloric is conducted through the body.

Let



Let M be a body (a mass of iron for instance) composed of an indefinite number of particles, arranged in the strata 1, 2, 3, 4, 5, 6, 7, &c. Let caloric be communicated to it in the direction X. The first stratum of particles 1 combines with a certain dose of this caloric, and forms a compound, which we shall call A. This compound cannot be decomposed by the second stratum, because all the strata, before the application of the heat, were at the same temperature; consequently, the affinity of all for caloric must have been equal. Now it would be absurd to suppose a compound destroyed by an affinity not greater than that which produced it. If, therefore, only one dose of caloric combined with stratum 1, no caloric could pass beyond that stratum. But the compound A has still an affinity for caloric; it therefore combines with another dose of it, and forms a new compound, which we shall call B. Now it is a general law, to which I know not a single exception, that when a body combines with different doses of another body successively, the first dose is retained by a stronger affinity than the second, the second by a stronger than the third, and so on. Thus iron, by combining with 25 per cent. of oxygen, forms the green oxide of iron: by combining with 48 per cent. it forms the brown oxide. Here, then, are two doses of oxygen which combine successively with the iron; the first is .28 per cent. the second 23 per cent. Now the second dose is not retained by an affinity nearly so great as the first dose. For many substances are capable of abstracting the second dose, and, consequently, of converting brown oxide of iron into green oxide which have no action on the green oxide: but all the bodies capable of decomposing the green oxide are capable also of decomposing the brown oxide.

Let us apply this general law to the compound B, into which stratum 1 has entered. This stratum is now combined with two doses of caloric, the first of which is retained by a stronger affinity than the second. Stratum 2, therefore, though incapable of decomposing the compound A, has a stronger affinity for caloric than compound A has for the second dose of caloric; it therefore seizes upon this second dose, combines with it, and forms the compound A. Here, now, are two strata of particles, combined each with a dose of caloric, and, consequently, constituting the compound A. The third stratum is unable to decompose the second, for the same reason that the second was unable to decompose the first. Stratum 1 again combines with a dose of caloric, and forms compound B: stratum 2 is unable to decompose this compound, because being already compound A, its affinity for caloric cannot be greater than that of compound A. Caloric, then, cannot pass farther through the body; but stratum 1 combines with a new dose of caloric, and forms a compound, which we shall call C. The affinity of this third dose being inferior to that of the second, stratum 2 decomposes compound C of stratum 1, and forms itself compound B. This compound is decomposed by stratum 3, which now forms compound A. Stratum 1 again forms compound C, which is again decomposed by stratum 2, which stratum forms a

new compound B. Compound C is a third time formed by stratum 1. Three strata are now heated. Stratum 1 is combined with three doses, stratum 2 with two doses, stratum 3 with one dose. The heat can pass no farther, for stratum 4 cannot decompose compound A, nor compound A compound B, nor compound B compound C; but stratum 1 combines with a fourth dose of caloric, and forms a new compound, which we shall call D. This new compound is decomposed by stratum 2, which forms compound C. Stratum 2 is decomposed by stratum 3, which forms compound B, and stratum 3 by stratum 4, which forms compound A. Stratum 1 again forms compound D, which is again decomposed by stratum 2. This, in its turn, is decomposed by stratum 3, which forms compound B. Stratum 1 again forms compound D, which is again decomposed by stratum 2, in order to form compound C. Compound D is again formed by stratum 1, and is not now decomposed. Here are four strata combined with caloric. Stratum 1 with four doses, stratum 2 with three doses, stratum 3 with two doses, and stratum 4 with one dose. In this manner may the heating progress go on till any number of strata whatever are combined with caloric.

Thus we see that caloric is conducted through bodies merely by repeated chemical combinations and decompositions. Hence the reason of the slowness of its progress. We see, too, that the temperature of the strata must diminish in a kind of arithmetical ratio according to their distance from the source of caloric; and this, in fact, holds in every instance. When one end of an iron rod is held in the fire, the temperature gradually diminishes as we approach the other extremity. We see, too, that the distance to which a body can conduct caloric must in all cases depend upon the degree of heat to which the first stratum of its particles can be subjected, before it changes its state. If it can bear a very great heat, the body will conduct to a very considerable distance; whereas, if it can bear but little heat, the body will conduct but a very short way. Thus brick will conduct much farther than wood, because when the temperature is raised above a certain degree, the wood catches fire, and is dissipated; whereas, the brick undergoes no change. The distance, then, to which a body conducts caloric, is not a criterion of the goodness of its conducting power, so much as of the degree of heat to which it can be subjected without a change of state, unless the heat be far inferior to what is capable of producing such a change. It appears, too, from this account, that the conducting power of a body is, in all cases, inversely as its affinity for caloric. Consequently, the best conductors are those bodies which have the weakest affinity for caloric.

It can scarcely be doubted that caloric is conducted through all *solid* bodies in the manner above described. All solid bodies hitherto examined are conductors of caloric. Fluids, too, were universally considered as conductors, till Count Rumford drew the contrary conclusion from his very ingenious experiments. If his conclusions be just, this constitutes a very curious and important distinction between solid and fluid bodies. But the more important the conclusion is, the more rigidly ought we to examine the premises before we adopt it.



Count Rumford has proved, by a set of very ingenious and happily-contrived experiments, that when heat is applied to a fluid, the fluid is heated in consequence of every individual particle going in its turn to the source of heat, combining with caloric, and then giving place to other particles, which combine with caloric in their turn. It is evident, then, that solid and fluid bodies are not heated in the same manner. The first set of bodies can only be heated by their conducting power, in the second set every individual particle goes directly to the source of heat. Though fluids, then, when placed upon the fire, acquire heat very speedily, we are not entitled to conclude, from this circumstance, that they are conductors of caloric. This point can only be determined by direct experiments.

The experiments which Count Rumford\* made for this purpose may be divided into two sets. The first set was made in order to show that when the internal motions of fluids are impeded, they are not heated so soon as when their internal motions are unimpeded. This, I think, he has very completely established. But as fluids are not heated by their conducting power, it does not follow, from these experiments, that fluids are non-conductors. They demonstrate merely that, if fluids be conductors, their conducting power is much inferior to the power by which they are heated. Count Rumford appears to have seen this himself, and to have looked upon these experiments rather as auxiliaries than as satisfactory proofs.

The second set of experiments was made in order to prove that caloric cannot pass *downwards* through fluids. Now, as caloric can pass downwards through all conducting bodies, and must indeed, from the very nature of the thing, pass in every direction through them, it follows that, if it cannot move downwards through fluids, they are, in fact, *non-conductors*. These experiments, then, are perfectly in point. Let us consider them a little.

To see whether caloric moves downwards, through water, he fixed a cake of ice in the bottom of a glass jar, covered  $\frac{1}{4}$  inch thick with cold water. Over this was poured gently a considerable quantity of boiling water. Now, if water were a non-conductor, no caloric would pass through the cold water, and consequently, none of the ice would be melted. The melting of the ice, then, was to determine whether water be a conductor or not. In two hours about half of the ice was melted. This, one would think, at first sight, a decisive proof that water is a conductor. But the Count has fallen upon a very ingenious method of accounting for the melting of the ice, "without being under the necessity," as he tells us, "of renouncing his theory, that fluids are non-conductors."

It is well known that the specific gravity of water at  $40^{\circ}$  is a maximum: if it be either heated above  $40^{\circ}$ , or cooled down below  $40^{\circ}$ , its density diminishes. Therefore, whenever a particle of water arrives at the temperature of  $40^{\circ}$ , it will sink to the bottom of the vessel. Now, as the water next the ice was at  $32^{\circ}$ , it is evident that whenever any part of the hot water was cooled down to  $40^{\circ}$ , it would sink, displace the water at  $32^{\circ}$ , come into

\* Count Rumford's experiments are detailed in our Journal; for which see the Indexes.---N.

contact with the ice, and, of course, melt it. The Count's ingenuity, never without resources, enabled him to prove completely that the ice employed in his experiment was actually melted in that manner. For when he covered the ice partially with slips of wood, that part which was shaded by the wood was not melted; and when he covered the whole of the ice with a thin plate of tin, having a circular hole in the middle, only the part exactly under the hole was melted. From these facts it certainly may be concluded that the ice was melted by descending currents of water.

But the point to be proved is not whether there were descending currents, but whether water be a conductor or not. Now, if water be a non-conductor, I ask how the hot water was cooled down to  $40^{\circ}$ ? Not at the surface; for the Count himself tells us, that there the temperature was never under  $108^{\circ}$ ; not by the sides of the vessel; for the descending current in one experiment was exactly in the axis; and it follows irresistibly from the experiment with the slips of wood, that these descending currents fell equally upon every part of the surface of the ice; which would have been impossible, if these currents had been cooled by the side of the vessel. The hot water, then, must have been cooled down to  $40^{\circ}$  by the cold water below it; consequently, it must have imparted caloric to this cold water. If so, one particle of water is capable of absorbing caloric from another; that is, water is a *conductor of caloric*. After the hot water had stood an hour over the ice, its temperature was as follows:

At the surface of the ice	-	-	-	-	-	$40^{\circ}$
1 inch above the ice	-	-	-	-	-	80
2 inches	-	-	-	-	-	118
3 inches	-	-	-	-	-	128
4 inches	-	-	-	-	-	130
7 inches	-	-	-	-	-	131

How is it possible to account for this gradual diminution of heat as we approach the ice, if water be a non-conductor? The water, it may be said, gives out caloric at its surface, falls down and arranges itself according to its specific gravity. If so, how comes it, that there is only one degree of difference between the temperature at 4 and at 7 inches above the ice? Thus it appears that the Count's experiment, instead of demonstrating that water is a non-conductor, rather favours the supposition that it is a conductor.

The Count drew, as a corollary, from this experiment, that water at  $41^{\circ}$  will melt as much ice in a given time as boiling water, when both stand over the ice. He found this actually to hold; or rather he concluded from his experiments, assisted, however, by a little calculation, that water at  $41^{\circ}$  will melt more ice in a given time than boiling water. It would not be difficult, perhaps, to find some flaws in the calculation; but granting the truth of the fact, I do not perceive how it contributes in the smallest degree to prove that water is a non-conductor. For water at  $41^{\circ}$  (being denser than water at  $32^{\circ}$ ) melts the ice, not by its conducting power; but by actually travelling to it particle after particle, and giving out its caloric: whereas, boiling water can only act by its conducting power,



till some part of it be cooled down to  $40^{\circ}$ . And as water is confessedly a very bad conductor, it is a very considerable time before much of it is cooled down so low. Accordingly we find that after 20' the boiling water at the distance of only one inch above the ice was  $130^{\circ}$ , and of half an inch  $46^{\circ}$ . Water at  $41^{\circ}$ , then, has an advantage over boiling water, and this advantage is the greater the worse a conductor of caloric the water is.

I shall not be so particular in my examination of Count Rumford's experiments made in order to prove that *mercury* and *oil* are non-conductors of caloric; because they are not susceptible of sufficient accuracy to decide the point. The iron cylinder would retain its heat too short a time to melt much ice, even if these fluids had been excellent conductors: and I really do not see how it was possible to detect a very small quantity of water, even if some of the ice had been melted. But if the Count had made use of a thermometer instead of ice, it would have risen several degrees, as I have found by making the experiment, even though the hot iron had been at a greater distance from the bulb, than his was from the nipple of ice.

It appears, then, that Count Rumford's experiments are not decisive, or if any conclusion can be drawn from them, it is rather favourable to the supposition that fluids are *conductors of caloric*. It occurred to me, that by varying Count Rumford's experiments a little, the point in question might be decided with certainty. I took a tubulated glass receiver M, Fig. 2, Pl. XXIII. into which there passed the thermometer B. This thermometer was so placed, that its bulb was exactly in the axis of the vessel. The mouth at which it entered was closed with cement. The degrees were marked upon the tube itself, and were all on that part of it which was without the receiver. Into this vessel I poured the fluid, whose conducting power I wished to examine, till it stood at a certain height above the thermometer B, suppose at D. I then poured cautiously over it (suppose to the height E) another fluid of inferior specific gravity, and heated to a certain temperature. Now, if the thermometer B did not rise, it would follow that the fluid was a non-conductor; if it did rise, that it was a conductor. A variety of precautions were necessary to prevent being misled; but these will appear from the experiments themselves, which I shall now detail.

The first fluid made choice of was mercury. I filled the vessel M with mercury till it stood 0.2 inch above the bulb of the thermometer B. Over this was poured cold water, till it stood  $\frac{1}{2}$  inch above the surface of the mercury. I then cautiously poured in boiling water till it stood about two inches above the surface of the mercury. The thermometer A was suspended in the water, so that its bulb approached very near the surface of the mercury. There was also a third thermometer C, whose bulb, which was oblong, rested against the bottom of the vessel. The temperature of the mercury and of the external air was  $42^{\circ}$ . That of the hot water, as soon as it could be observed after pouring it in (which took up some time), was  $174^{\circ}$ . The result of the experiment was as follows:

## EXPERIMENT

## EXPERIMENT I.

*Thermometer B rose to 80° during the pouring in of the water.*

Time.	Therm. A.	Therm. B.	Therm. C.	Water $\frac{1}{2}$ inch below the surface.
In 2'	126°	85°	67°	174°
5	111	90	83	163
10	106	93 $\frac{1}{2}$	88	152
12		95		
15	104	96	91	144
21	102	96	91 $\frac{3}{4}$	133
25	100	96	91 $\frac{3}{4}$	128
28	98	95	91 $\frac{3}{4}$	122
35	97	93	89 $\frac{1}{2}$	116
40	95	91	88	100
45	93	90	87	106
50	91	88	85	102
55	88	86	84	99
60	86	85	82	96
65	84	83 $\frac{1}{2}$	81	92
70	83	82	79	90
75	80	80	78	88
80	79	78	76	85
85	77 $\frac{1}{2}$	77	75	83
90	76	76	73	80
95	74	74	72	76

Here the thermometer B rose in 15 minutes 16°; and in 21' the thermometer C rose 24.75°: all this time the thermometer A was gradually falling, and approaching nearer and nearer the temperature of B, and after 95' the two were precisely of the same temperature. The maximum of B was 96°, of C 91.75°. B first acquired its maximum, then C: B began to fall in 25', C in 28'. Now all these circumstances are precisely what ought to happen if mercury be a conductor of caloric. The experiment, therefore, seems to prove that mercury actually conducts caloric: for we see that caloric passes downwards through mercury, which could not happen if mercury were a non-conductor.

Though this experiment seemed decisive, I wished to repeat it in a different manner, in order, if possible, to determine the internal motions of the hot liquid, that I might ascertain whether or not these motions had any influence on the rise of the thermometers. Count Rumford's ingenious method readily presented itself. Accordingly I poured over the mercury a saturated solution of carbonate of pot-ash in water, till it stood  $\frac{1}{2}$  inch above its surface. Then boiling carbonate of pot-ash was cautiously poured over all, till it stood



two inches above the surface of the mercury. As this solution had not been filtered, there were floating through it a number of opaque particles, nearly of the same specific gravity with the solution. By the motion of these I expected to be able to judge of the currents in the hot liquid. The temperature of the mercury and of the external air was  $41\frac{1}{2}^{\circ}$ . That of the carbonate, when first tried after being poured in,  $174^{\circ}$ . The result of the experiment was as follows:

## EXPERIMENT II.

*Thermometer B rose during the pouring in of the carbonate to  $75^{\circ}$ .*

Time.	Therm. A.	Therm. B.	Therm. C.
In	$118^{\circ}$	78	$55^{\circ}$
1	109	80	67
2	103	85	78
6	100	88	84
10	98	90	85
15	96	90	87
20	94	90	86
25	92	89	86
30	90	87	85
35	87	85	83
40	85	84	82
45	84	83	80
50	82	80	$78\frac{1}{4}$
55	80	$79\frac{1}{2}$	77
60	78	77	$75\frac{1}{2}$
65	$76\frac{1}{2}$	76	74
70	75	$74\frac{1}{2}$	$73\frac{1}{2}$
75	73	73	$70\frac{1}{2}$
80	72	72	70
85	70	70	$68\frac{1}{2}$
90	69	69	67
95	68	$67\frac{1}{2}$	66
100	$66\frac{1}{2}$	66	65
105	65	65	64
110	$64\frac{1}{2}$	64	63
115	63	63	62
120			

Here the result is almost the same as in experiment first. It is therefore a confirmation of it, and makes the conclusion drawn from it, that mercury conducts caloric, more to be depended on.

Soon after pouring in the hot carbonate, I observed the opaque particles formerly mentioned, moving pretty rapidly upwards and downwards, indicating Count Rumford's currents. These motions did not depend upon the sides of the vessel, for particles were ascending and descending in every part of the liquid. They ascended with great rapidity till they came within a certain distance of the surface; then they descended again as rapidly as they had ascended, till they came within a certain distance of the mercury; here they became stationary for a moment, and then ascended again with as much velocity as at first. Sometimes a few particles ascended to the very surface, and then descended again with very great rapidity. But they constantly stopped, and began to move upwards when they came to a certain distance from the mercury; and they all stopped at the same distance from the mercury. At first this distance was about half an inch, but it became gradually greater as the process of cooling went on, and at last the particles did not descend lower than the middle of the liquid. If any particle happened to go beyond this boundary, and to approach a little nearer the mercury, it soon became stationary, and did not rise any more. The rapidity of the motions of these particles gradually diminished, as well as the space through which they moved: after 40' I could not observe them moving at all; though, when I marked the situation of a particle, and then observed it some time after, it became evident that it had changed its place.

These motions are curious, and give us a good deal of information about the process of cooling in the hot liquid. I pretend not to explain their rapidity; but, waving that difficulty, it seems pretty clear that the particles near the surface giving out their heat, became specifically heavier, and tumbled down towards the mercury. They did not reach the mercury, however, because at some distance from it they came to a part of the liquid colder than themselves. If these descending particles happened, in consequence of a momentum accidentally greater than usual, to penetrate into this cold stratum, they lost their excess of caloric before they could rise again, and, of course, continued ever after stationary in the cold stratum. These descending currents necessarily produced ascending currents; but these ascending particles could scarcely reach the surface, unless their momentum happened to be unusually great, because the water being hottest just below the surface, they soon came to particles specifically lighter than themselves. As the cooling advanced, the cold stratum, of course, increased, and the currents moved more slowly, because the water at the surface, approaching to the temperature of the air, was cooled more slowly. We see, then, that the descending currents do not reach the mercury; consequently, they cannot affect its heating, except indirectly; far less can they affect the temperature of the thermometer.

Wishing, now, to try a hot liquid of a different kind, I poured lintseed oil, at  $230^{\circ}$ , over the mercury, till it stood an inch and a half above its surface. The temperature of the mercury, and of the external air, was  $41\frac{1}{2}^{\circ}$ ; that of the oil, as soon as it could be ascertained, after pouring it in, was  $180^{\circ}$ . The result was follows:

#### EXPERIMENT



## EXPERIMENT III.

*Thermometer B rose during the pouring to 50°.*

Time.	Therm. A.	Therm. B.	Therm. C.
2'		56°	50°
5	91°	60	56
10	82	63	60 $\frac{1}{2}$
15	77 $\frac{1}{2}$	64	63
20	74	64 $\frac{1}{2}$	62 $\frac{3}{4}$
25	70	64 $\frac{1}{2}$	62 $\frac{3}{4}$
30	67 $\frac{1}{4}$	63 $\frac{1}{2}$	62
35	67 $\frac{1}{2}$	63	61 $\frac{1}{2}$
40	64	62	60 $\frac{3}{4}$
45	62 $\frac{1}{2}$	61	59 $\frac{3}{4}$
50	61	60	58 $\frac{3}{4}$
55	59 $\frac{1}{2}$	58 $\frac{1}{2}$	57 $\frac{1}{4}$
60	58 $\frac{1}{2}$	58	57
65	57	57	56 Oil
70	56	56	55 58

Here the general result is precisely the same as in the former experiments. The only remarkable difference is the small rise of the thermometer during the pouring in of the hot oil. During that operation it rose only  $8\frac{1}{2}^{\circ}$ , whereas in the two former experiments its rise was nearly  $30^{\circ}$ .

These three experiments were varied different ways, but the results were so nearly the same, that it would be both useless and tiresome to relate the whole. They seem to me to put it beyond a doubt that mercury is a conductor of caloric.

I wished now to try whether water also be a conductor. For this purpose I poured water at  $40^{\circ}$  (the temperature of the air) into the vessel M, till it stood 0.8 inch above the bulb of the thermometer B. Over this I poured a thin stratum of lintseed oil. Then lintseed oil, heated to  $220^{\circ}$ , was poured cautiously over all, till it stood 1.15 inches above the surface of the water. The temperature of the oil, as soon as it could be ascertained after it had been poured in, was  $160^{\circ}$ . The result of the experiment was as follows :

2'	82	52	62
5	72	52	62
10	62	52	62
15	52	52	62
20	42	52	62
25	32	52	62
30	22	52	62
35	12	52	62
40	2	52	62

3 Z 2

EXPERIMENT

## EXPERIMENT IV.

*Thermometer B rose during the pouring 0°.*

Time.	Therm. A.	Therm. B.	Therm. C.
In 5'		41°	
10	79°	42	
15	70	43	42½
20	68	44	43
25	65	45	43
30	63	46	43
35	61	46½	43
40	59	46½	43
45	57	46½	43½

We see from this experiment that water is a conductor of caloric; but a much worse conductor than mercury. The thermometer B did not rise during the pouring in of the oil, partly because it was at a considerable distance from the surface, and partly because water conducts worse than mercury. But it did rise afterwards 6½°. It was 5' before it began to rise, and 35' before it reached its maximum. The result of this experiment was so curious, that I wished to repeat it with some variation. I put all the three thermometers in the water: thermometer A 0.2 inch below the surface, thermometer B 0.7 inch below the surface, and thermometer C 1.725 inches below the surface. The cold water was 2.2 inches deep; its temperature was 42° (that of the air). A stratum of cold lintseed oil was poured over the water, then lintseed oil, at 220°, was poured over all, till it stood 2.125 inches above the surface of the water. The result was as follows:

## EXPERIMENT V.

Time.	Therm. A.	Therm. B.	Therm. C.
In 2'	58°	45°	
5	59	46	42
10	60	49	42.75
15	60	50	43
20	60	51	43.5
25	59	51½	44
30	59	52	44.5
35	58	52	45
40	57	52	45.5
45	56½	52	46
50	56	52	46.5
55	55½	51½	46.75

Time.



Time.	Therm. A.	Therm. B.	Therm. C.
In 60	55°	51½°	47°
65	54	51¼	47.25
70	53½	51	47.5
75	53	51	48
80	53	51	48
85	52½	50½	48
90	52¼	50¼	48.25
95	52	50½	48.25
100	51½	50	48.25
105	51	50	48.5
110	51	49½	48.5
115	51	49¼	48.5
120	50½	49	48.25
125	50	48¾	48.25
130	50	48¾	48.25
223	47	46	46

Here the thermometer A rose 18° and reached its maximum in 11'

B 10 31

C 6.5 106

This experiment seems to me to prove, in a very clear manner, that water is a conductor. But before we draw any such conclusion from it, or from those before detailed, it will be proper to enquire whether the rising of the thermometers can be accounted for in any other way. If it cannot, then water and mercury must be considered as conductors. Now, it may be said, that the thermometers rose in consequence of the action of *transmitted* caloric; or that caloric was *conducted* to them by means of the glass vessel in which the experiment was made; or that *currents* of the hot liquid made their way to them; or, lastly, that the cold liquid was thrown into such *agitation* by the pouring in of the hot liquid over it, that currents of it heated directly by the contact of the hot liquid made their way to the thermometers. If none of these suppositions will account for the rise of the thermometers, we must conclude from the experiments that mercury and water are conductors of caloric.

1. If the thermometers had risen in consequence of caloric transmitted to them through the cold liquids, they ought to have begun to rise at the same instant; or, rather, the lowest, which was the most delicate, ought to have begun to rise first. But the contrary is evident from all the experiments. In the last experiment, for instance, the thermometer B did not begin to rise till some time after the oil had been poured in; and thermometer C not till more than 6' after I had begun to observe the thermometers, and, consequently, not till at least 12' after beginning to pour in the oil. Thermometer B reached its maximum in

31".

31', C in 106'; a length of time absolutely incompatible with every idea respecting transmitted caloric. It is not possible, then, that the thermometers could have risen in consequence of the action of transmitted caloric.

2. If the caloric had been conducted to the thermometers by the vessel, C, which was always nearest the vessel, ought to have risen first, or at least, as its situation was precisely the same, it ought to have risen the same number of degrees in experiments 3d and 4th. In these the hot oil was the same; it ought therefore to have communicated the same quantity of caloric to the vessel: but in experiment 3d, C rose both sooner and higher than in experiment 4th. This is easily explained, on the supposition that water is a worse conductor than mercury; but it is inexplicable, on the supposition that the heat was communicated by the vessel. The following experiment proves clearly that this supposition is without foundation.

To determine whether the cold liquid was thrown into waves, or whether currents were formed in it either by pouring the hot liquid over it, or during the process of heating, I took a solution of carbonate of pot-ash, with amber floating through it, prepared in the way described by Count Rumford, and poured it into the vessel M till it stood 0.3 inch above the thermometer B. Over this was poured cold water to the height of half an inch. The two surfaces kept perfectly distinct, as was evident from the amber, some of which was floating at the surface of the carbonate. Over all I poured boiling water, tinged blue with the juice of red cabbage. The blue colour hung down in clouds through rather more than the half of the pure water; but none of it approached the carbonate. The temperature of the carbonate was 40° (that of the air). Thermometer B rose during the pouring to 73°. Thermometers A and C placed as in experiment 1st. The result of this experiment was as follows:

#### EXPERIMENT VI.

Time.	Therm. A.	Therm. B.	Therm. C.
In	105°	75°	43°
4	102	78	46
10	98	79	47½
15	96	80	50
20	93	80	53
25	90	79	53½
30	88	78	55
35	86	77	56
40	83	76½	57
45	81	76	58
50	79½	75	58½
55	78	74	58¾
60			

Time.



Time.	Therm. A.	Therm. B.	Therm. C.
In 65	76°	73°	59°
70	74	72	59
75	72	70	59
80	71½	69	59
127	62	62	56
130	61¾	61¾	56
135	61	60½	56

During the whole of this experiment the amber continued perfectly motionless; consequently there were no currents. But there must have been currents, if the heat had been communicated by the sides of the vessel; for in that case the particles of the liquid next the sides must have received heat: this would have caused them to ascend, and there would have been descending currents in the interior parts of the liquid. It is not possible, then, to suppose that the thermometers rose in consequence of caloric conducted to them by the sides of the vessel.

3. If the thermometers had risen in consequence of currents of the hot liquid striking against them, these currents would have been visible in several of the experiments. If the smallest particle of oil, for instance, had made its way through the water, it would have been detected by its whiteness. We are absolutely certain that no such currents existed in the 6th experiment; for had the smallest atom of the water, tinged with red cabbage, made its way through the carbonate, it would have been instantly detected by a change of colour from blue to green. But no such change of colour took place: nay, more than six hours after the experiment, no green colour could be perceived; but it appeared instantly after a very slight agitation. Besides, how could oil or water make its way downwards through mercury? This supposition, then, must also be given up.

4. If the cold liquid had been so much agitated by pouring the hot liquid over it, that currents of it, heated directly by the hot liquid, made their way to the thermometers, these currents must have been visible in the 6th experiment, which was made on purpose to detect them. Now, as the amber remained perfectly motionless, even during the pouring in of the hot liquid, and never moved after, it follows that no such currents existed. Consequently, the fourth supposition cannot be admitted.

Upon the whole, then, I consider these experiments as proving, beyond a doubt, that water and mercury are conductors of caloric. I have repeated them frequently, in order to guard as much as possible against mistakes: but I trust that others, who are interested in this subject, will take the trouble to repeat them. I may, perhaps, have overlooked something, notwithstanding all my caution. If I have, the experiments are so easily made, that it cannot fail to be very soon discovered. Whatever may be the issue of this investigation, the greatest part of the merit belongs exclusively to Count Rumford. My experiments were made merely in consequence of his, and I availed myself of the contrivances which he himself had suggested.

To see whether fulphuric acid be a conductor of caloric, I made the following experiment:

EXPERIMENT

## EXPERIMENT VII.

Into the vessel M was poured concentrated sulphuric acid, till it stood 0.6 inch above the thermometer B. The temperature was  $43^{\circ}$  (that of the air). Over this was poured cautiously a saturated aqueous solution of sulphate of pot-ash with excess of acid, at  $43^{\circ}$ , till it stood 0.4 inch above the acid. The two liquids remained perfectly distinct, as was evident from the difference of their colours and consistence; for the acid was a little coloured. The surface where they joined reflected light very strongly. The thermometer A was suspended, so that its whole bulb was in the sulphate; thermometer C was 1.366 inches under the surface of the acid; the acid began slowly to combine with the water above it, and this combination gradually evolved heat. I expected that this heat would make its way downwards, and raise the thermometers. The result was as follows:

Time.	Therm. A.	Therm. B.	Therm. C.
In 3'		$49^{\circ}$	
5		50	
10		54	
15		56	$47^{\circ}$
20	$82^{\circ}$	58	$47\frac{1}{4}$
25	82	60	49
30	82	$60\frac{1}{4}$	50
35	82	61	51
40	81	$61\frac{1}{2}$	$51\frac{3}{4}$
45	80	62	$52\frac{1}{4}$
50	$79\frac{1}{2}$	$62\frac{1}{2}$	$52\frac{3}{4}$
55	79	63	$53\frac{1}{2}$
60	$78\frac{1}{2}$	63	54
65	$77\frac{1}{2}$	63	$54\frac{1}{2}$
70	77	$63\frac{1}{2}$	55
75	$76\frac{1}{2}$	$63\frac{1}{2}$	$55\frac{1}{4}$
80	76	63	$55\frac{3}{4}$
85	75	63	56
90	74	63	$56\frac{1}{4}$
95	$73\frac{1}{2}$	$62\frac{1}{2}$	$56\frac{1}{4}$
100	73	$62\frac{1}{2}$	$56\frac{1}{4}$
105	$72\frac{1}{2}$	$62\frac{1}{2}$	$56^{\circ}$
110	72	62	$56\frac{1}{4}$
115	$71\frac{1}{2}$	62	$56\frac{1}{4}$
120	71	$61\frac{3}{4}$	$56\frac{1}{4}$
125	70	$61\frac{3}{4}$	56
130	70	$61\frac{1}{2}$	56
230	62	58	53



Two days after this experiment was made, the surfaces of the acid and sulphate were still as distinct as ever; but the stratum of sulphate was nearly double its original thickness, owing evidently to its having combined with a considerable portion of the acid. This experiment, I think, proves that sulphuric acid is also a conductor of caloric. Indeed, it is more than probable that all fluids are conductors.

## II.

*Description of a Steam Engine on the Principle of Savary, operating by a separate Condenser; with other essential Improvements. By Mr. JOHN NANCARROW\*.*

- P** LATE XXIII. Fig. 3. A. The receiver, which may be made either of wood or iron.  
 BBBB. Wooden or cast-iron pipes for conveying the water to the receiver, and from thence to the penstock.  
 C. The penstock or cistern.  
 D. The water-wheel.  
 E. The boiler, which may be either iron or copper.  
 F. The hot-well for supplying the boiler with water.  
 GG. Two cisterns under the level of the water, in which the small bores BB, and the condenser are contained.  
 HHH. The surface of the water with which the steam-engine and water-wheel are supplied.  
 aa. The steam-pipe, through which the steam is conveyed from the boiler to the receiver.  
 b. The feeding-pipe, for supplying the boiler with hot water.  
 cccc. The condensing apparatus.  
 dd. The pipe which conveys the hot water from the condenser to the hot-well.  
 eee. Valves for admitting and excluding the water.  
 ff. The injection pipe, and g the injection cock.  
 h. The condenser.

It does not appear necessary to say any thing here on the manner in which this machine performs its operations without manual assistance, as the method of opening the cocks by which the steam is admitted into the receiver and condensed, has been already well described by several writers. But it will be necessary to remark that the receiver, penstock, and all the pipes, must be previously filled † before any water can be delivered on the

\* From a learned paper on Mills, in the American Transactions, IV. page 355. I have not been able to extract the theoretical parts of the paper, because the plates and references are too imperfect.

† Or the air blown out by steam; which may perhaps be less convenient than the method in the text.--N.

wheel; and when the steam in the boiler has acquired a sufficient strength, the valve at *c* is opened, and the steam immediately rushes from the boiler at *E* into the receiver *A*; the water descends through the tubes *A* and *B*, and ascends through the valve *e* and the other pipe or tube *B* into the penstock *C*. This part of the operation being performed and the valve *c* shut, that at *a* is suddenly opened, through which the steam rushes down the condensing pipe *c*, and in its passage meets with a jet of cold water from the injection cock *g*, by which it is condensed. A vacuum being made by this means in the receiver, the water is driven up to fill it a second time through the valves *ee*, by the pressure of the external air, when the steam-valve at *c* is again opened, and the operation repeated for any length of time the machine is required to work.

There are many advantages which a steam-engine on this construction possesses, beyond any thing of the kind hitherto invented; a few of which I shall beg leave to enumerate.

1. It is subject to little or no friction.
2. It may be erected at a small expence when compared with any other sort of steam-engine.
3. It has every advantage which may be attributed to Boulton and Watt's engines, by condensing out of the receiver, either in the penstock or at the level of the water.
4. Another very great advantage is, that the water in the upper part of the pipe \* adjoining the receiver, acquires a heat by its being in frequent contact with the steam, very nearly equal to that of boiling water; hence the receiver is always kept uniformly hot as in the case of Boulton and Watt's engines.
5. A very small stream of water is sufficient to supply this engine, (even where there is no fall) for all the water raised by it is returned into the reservoir *HHH*.

From the foregoing reasons it manifestly appears that no kind of steam-engine is so well adapted to give rotatory motion to machinery of every kind as this. Its form is simple, and the materials of which it is composed are cheap; the power is more than equal to any other machine of the kind, because there is no deduction to be made for friction, except on account of turning the cocks, which is but trifling.

Its great utility is therefore evident in supplying water for every kind of work performed by a water-wheel, such as grist-mills, saw-mills, blast-furnaces, forges, &c.

\* Not being thrown out by a side aperture, as in Plate *XVII*, Vol. *I*, of our Journal, but merely raised and depressed in the pipe *AB*.---N.



## III.

*New Application of the Syphon to raise Water above the Surface of the Reservoir. In a Letter from Mr. WILLIAM CLOSE.*

TO MR. NICHOLSON.

S I R,

THE syphon will raise a stream of water through an extensive space in every situation where a little descent can be procured, but while the operation continues, no water can be taken directly out of the stream above the lowest part of the tube. When, however, the two open ends of a syphon are closed, a quantity of water may be let out of the highest part, and its place supplied by introducing a like quantity of no use: all the avenues for the purpose being then closed, and the stream suffered to flow through the tube; the useless water will be displaced, and a fresh quantity may be soon after drawn off. This mode of exchange may be useful in furnishing a supply for washing, and some other purposes, but there are several domestic uses for which the water drawn off will not be thought sufficiently pure. A method of taking water out of the syphon at any height within the limits of the elevation, without retarding the stream, or introducing another quantity, has often appeared to me very desirable, and some months ago, I made a number of experiments to determine the practicability of the project.

My hopes of success were founded upon the following observations:

When water flows through a syphon that has an elbow or dilatation containing air, the stream moves much slower, and is smaller on entering into the external air, than when the part is completely filled with water. When the current moves with a certain velocity, its lateral action extracts the air, and carries it down the descending column. As the part fills with water, the stream becomes fuller, and its velocity encreases.

Among a variety of projects for extracting the air out of a vessel fixed to the side of a syphon, and filling it with water, by the assistance of the lateral action of the stream, the following seemed the most likely:

If the connection between the vessel and syphon be made by two tubes, the end of one turned against the current, and the end of the other turned from it, the stream after striking the end of the first tube, will be contracted until it has passed by the end of the second. After this it will begin to expand; the air will be drawn out of the vessel through the tube turned from the current, and water will enter by the other, until the vessel is filled; the stream beyond the contraction occasioned by the two tubes will then receive a supply through the vessel.

The result of an experiment founded upon this theory was very satisfactory. In place, however, of a particular account of the first experiment, I shall give a detail of some others, in which a still more easy expedient was adopted, to produce the same effect:

Two small demi-cylindrical tubes when constructed were joined together in such a manner, that one piece served for the flat sides of both. They were folded into the side of a syphon at the middle of the turn, or that part which the water passes through at its highest elevation. The flat partition between them was thin, and projected a little into the inside of the syphon, but their semi-circular sides had no projection at all. When the branches of the syphon were held horizontally, the position of the two small tubes was vertical, and the plane of the partition was parallel to a line drawn between the branches. The small cylinder formed by both tubes was then encircled with thread to a proper thickness, and inserted into the neck of an ounce glass vial.

Every part of this apparatus being perfectly air tight, the syphon was filled with water, by holding it in nearly an horizontal position below the surface of water in a capacious vessel: both of its apertures were then closed by the thumbs of the operator, and the branches were set in a vertical position: the end of one branch being immersed in water, its aperture was opened, and the efflux through the tube was permitted immediately after, by opening the aperture of the other branch.

When the stream had a descent in the tube equal to one inch below the surface of the water in the vessel, bubbles of air were ejected, and in a short time the bottle was filled with water to a level with the upper sides of the two small tubes; a continued circulation was then visible. The water entered the bottle through the tube on that side of the partition which was struck by the stream, and passed out through the tube on the other side, with a velocity proportionate to the active weight of the descending column, which was increased or diminished at pleasure, by lowering or raising the syphon. When the effective space of descent was less than one inch, no air was extracted out of the bottle.

In this experiment the length of each branch of the syphon was seven inches: the diameter of the tube was all throughout equal to about half an inch.

The length of one branch of the syphon being increased to forty-five inches, and that of the other to forty-three, the ounce bottle was changed for one of eight ounces, and the syphon was filled with water in an inverted vertical position. After its two apertures were respectively closed with corks, it was laid horizontally, and the bottle was detached from it, to let out a little water which had been forced into it; and also with an intent to lessen the compression of the air it contained. After replacing the bottle, the syphon was set into its proper vertical position, with its shorter branch plunged in water, and the efflux was permitted by drawing the corks. When the shorter column was equal to forty-one inches and an half, it was requisite to settle the syphon a few inches immediately at the commencement of its operation, to prevent the longer column from wasting so much: as to let the shorter preponderate. When the stream began to extract the air, the syphon was raised to its former elevation, or even higher, and water continued to enter the bottle, and air to be carried down the descending column, until the shorter branch could no longer receive a supply of water. The active length of the descending column was then no longer than two inches and an half.

It



It was not necessary to settle the syphon when only an ounce bottle was used.

When the syphon was in its original short state, the partition between the two small pipes was taken smoothly off with the inside of the tube. No water would enter the bottle, although the active part of the descending column was of considerable length.

From these facts and observations I deduce the arrangement represented in Fig. 1; Plate XXIV. for extracting a quantity of water out of a syphon at any elevation, and supplying its place with air.

Into any part, except the top side, of a vertical syphon S Y, insert two small pipes, and let their apertures in the inside of the tube be divided by a projecting piece about a quarter of an inch thick; wherever the pipes are inserted, the piece must be placed in such a position, that the current will strike against one of its flat sides. The pipe which opens on that side of the obstacle or dam struck by the stream, may be called the water pipe; and that on the other side the air pipe. Insert their other ends into a vessel A W. The air pipe opposite to A must rise to near the top of this vessel, but the water pipe W need not rise above the place of its insertion. A cock perfectly air tight must be fixed in each pipe between the vessel and syphon: the vessel A W must have a tube in its lower part for letting out water. This tube must have a cock fixed in it, or a valve covered with leather to close its lower end. To hasten the delivery of the water in this vessel, the external air may be admitted in such manner as is most convenient.

The communication between the vessel and syphon being intersected by turning the cocks in the pipes A W, and the branches of the syphon closed at their lower ends, the tube may be filled with water through an aperture in the top. After this aperture is closed, and a stream of water let into the cistern C for supplying the syphon, the ends of the branches may be opened, and a continued stream will flow through the tube.

When it is required to fill the vessel A W with water, exclude the external air, and open the pipes between it, and the syphon. The vessel will soon be filled, and the water may be let out by opening the tube for that purpose, after the small pipes A W are again closed by turning their cocks.

The water may be let out of the vessel without attendance, by a quantity of water passing through four vessels placed in the following order one below another, and each provided with a syphon.

1. The highest, an immoveable vessel filled in a given time.
2. A descending vessel, suspended from a lever or wheel, which turns the cocks in the tubes opposite A W in its axis. This vessel must have a tube open at both ends, fixed in the middle of its bottom.
3. A descending vessel to open the valve for letting water out of the vessel A W. It must be suspended upon the valve by a cord or wire passing through the tube in the middle of the second vessel.
4. The lowest, a vessel of the same width with the second. The brim of it must be connected to the outside circumference of the bottom of the second by wires or chains.

In this arrangement the first vessel will empty itself into the second, which will close the cocks in the pipes opposite A and W, before air is admitted into the vessel A W. The third will be filled from the second, and the water in the vessel A W will be let out. Again, the third will deliver its contents in the fourth or lowest, which will keep the cocks in the small pipes opposite A and W close, until after the third vessel is empty, has risen up, and the external air can no longer enter the vessel A W. The fourth being then emptied by its syphon, the pipes between the vessel A W and syphon S Y will open.

The diameter of the second vessel should be something less than either that above or below it. The fourth should be filled before the second is empty; the third will descend the last and rise the first: the second and fourth will rise together immediately after the third. If the second and fourth were to rise before the third, the syphon would directly receive a quantity of external air, and its operations would cease. It will therefore require much caution to manage the cocks and valves, if another vessel, similar to A W, is to be filled while this last is emptied, and emptied while it is filled.

The vessel A W should not be large, and in order to overcome the buoyance of the extracted air, it is adviceable to make the length of the descending branch of the syphon exceed the length of the ascending one as much as circumstances will admit, and to let the lowest part of it be made of a conical divergent form. The velocity of the stream will be thus encreased; the vessel A W will be sooner filled with water; and the depression of the two columns will be less liable to happen from very slight imperfections of workmanship.

I am, SIR,

Your humble servant,

Dalton, Feb. 17, 1801.

WILLIAM CLOSE.

#### IV.

*On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations. By WILLIAM HERSCHEL, LL. D. F. R. S.*

(Continued from page 506.)

WE may now proceed to determine the powers of the instruments that have been used in my astronomical observations; but, as this subject will be best explained by a report of the observations themselves, I shall select a series of them for that purpose, and relate them in the order which will be most illustrating.



First, with regard to the eye, it is certain that its power, like all our other faculties, is limited by nature, and is regulated by the permanent brightness of objects; as has been shewn already, when its extent with reflected light was compared to its exertion on self-luminous objects. It is further limited on borrowed light, by the occasional state of illumination; for, when that becomes defective at any time, the power of the eye will then be contracted into a narrower compass; an instance of which is the following:

In the year 1776, when I had erected a telescope of 20 feet focal-length, of the Newtonian construction, one of its effects by trial was, that when towards evening, on account of darkness, the natural eye could not penetrate far into space, the telescope possessed that power sufficiently to shew, by the dial of a distant church steeple, what o'clock it was, notwithstanding the naked eye could no longer see the steeple itself. Here I only speak of the penetrating power; for, though it might require magnifying power to see the figures on the dial, it could require none to see the steeple. Now the aperture of the telescope being 12 inches, and the construction of the Newtonian form, its penetrating power,

when calculated according to the given formula, will be  $\frac{\sqrt{429 \times 120^2 - 15^2}}{2} = 38,99$ .

*A*, *b*, and *a*, being all expressed in tenths of an inch\*.

From the result of this computation it appears, that the circumstance of seeing so well, in the dusk of the evening, may be easily accounted for, by a power of this telescope to penetrate 39 times farther into space than the natural eye could reach, with objects so faintly illuminated.

This observation completely refutes an objection to telescopic vision, that may be drawn from what has also been demonstrated by optical writers: namely, that no telescope can shew an object brighter than it is to the naked eye. For, in order to reconcile this optical theory with experience, I have only to say, that the objection is intirely founded on the same ambiguity of the word brightness that has before been detected. It is perfectly true, that the *intrinsic* illumination of the picture on the retina, which is made by a telescope, cannot exceed that of natural vision; but the *absolute* brightness of the magnified picture by which telescopic vision is performed, must exceed that of the picture in natural vision, in the same ratio in which the area of the magnified picture exceeds that of the natural one; supposing the *intrinsic* brightness of both pictures to be the same. In our present instance, the steeple and clock-dial were rendered visible by the increased absolute brightness of the object, which in natural vision was 15 hundred times inferior to what it was in the telescope. And this establishes beyond a doubt, that telescopic vision is performed by the absolute brightness of objects; for, in the present case, I find by computation, that the *intrinsic* brightness, so far from being equal in the telescope to that of natural vision, was inferior to it in the ratio of three to seven.

\* I have given the figures, in all the following equations of the calculated penetrating powers, in order to shew the constructions of my instruments to those who may wish to be acquainted with them.

The distinction between magnifying power, and a power of penetrating into space, could not but be felt long ago, though its theory has not been inquired into. This undoubtedly gave rise to the invention of those very useful short telescopes called night-glasses. When the darkness of the evening curtails the natural penetrating power, they come in very seasonably, to the relief of mariners that are on the look-out for objects which it is their interest to discover. Night-glasses, such as they are now generally made, will have a power of penetrating six or seven times farther into space than the natural eye. For, by the construction of the double eye-glass, these telescopes will magnify 7 or 8 times; and the object glass being  $2\frac{1}{2}$  inches in diameter, the breadth of the optic pencil will be  $3\frac{1}{8}$  or  $3\frac{7}{8}$  tenths of an inch. As this cannot enter the eye, on a supposition of an opening of the iris of 2 tenths, we are obliged to increase the value of  $a$ , in order to make the telescope have its proper effect. Now, whether nature will admit of such an enlargement becomes an object of experiment; but, at all events,  $a$  cannot be assumed less than

$\frac{A}{m}$ . Then, if  $x$  be taken as has been determined for three refractions, we shall have

$$\frac{\sqrt{853 \times 25^2}}{a} = 6,46 \text{ or } 7,39.$$

Soon after the discovery of the Georgian planet, a very celebrated observer of the heavens, who has added considerably to our number of telescopic comets and nebulae, expressed his wish, in a letter to me, to know by what method I had been led to suspect this object not to be a star, like others of the same appearance. I have no doubt but that the instrument through which this astronomer generally looked out for comets, had a penetrating power much more than sufficient to shew the new planet, since even the natural eye will reach it. But here we have an instance of the great difference in the effect of the two sorts of powers of telescopes; for, on account of the smallness of the planet, a different sort of power, namely, that of magnifying, was required; and, about the time of its discovery, I had been remarkably attentive to an improvement of this power, as I happened to be then much in want of it for my very close double stars\*.

On examining the nebulae which had been discovered by many celebrated authors, and comparing my observations with the account of them in the *Connaissance des Temps* for 1783, I found that most of those which I could not resolve into stars with instruments of a small penetrating power, were easily resolved with telescopes of a higher power of this sort; and, that the effect was not owing to the magnifying power I used upon these occasions, will fully appear from the observations: for, when the closeness of the stars was such as to require a considerable degree of magnifying as well as penetrating power, it always appeared plainly, that the instrument which had the highest penetrating power resolved them best, provided it had as much of the other power as was required for the purpose.

\*Magnifying powers of 460, 625, 932, 1159, 1504, 2010, 2398, 3168, 4294, 5489, 6450, 6652, were used upon  $\gamma$  Bootis,  $\gamma$  Leonis,  $\alpha$  Lyrae, &c. See Cat. of double stars, Phil. Trans. Vol. LXXII. page 115, and 147; and Vol. LXXV, page 48.



Sept. 20, 1783; I viewed the nebula between Flamsteed's 99th and 105th Piscium, discovered by Mr. Mechain, in 1780.

"It is not visible in the finder of my 7-foot telescope; but that of my 20-foot shews it."

Oct. 28, 1784, I viewed the same object with the 7-foot telescope.

"It is extremely faint. With a magnifying power of 120, it seems to be a collection of very small stars: I see many of them."

At the time of these observations, my 7-foot telescope had only a common finder, with an aperture of the object glass of about  $\frac{3}{4}$  of an inch in diameter, and a single eye-lens;

therefore its penetrating power was  $\frac{\sqrt{.899 \times 7.51^2}}{2} = 3.56$ . The finder of the 20-foot instrument, being achromatic, had an object glass 1.17 inch in diameter: its penetrating power, therefore, was  $\frac{\sqrt{.85 \times 11.71^2}}{2} = 4.50$ .

Now, that one of them shewed the nebula and not the other, can only be ascribed to space-penetrating power, as both instruments were equal in magnifying power, and that so low as not to require an achromatic object glass to render the image sufficiently distinct.

The 7-foot reflector evidently reached the stars of the nebula; but its penetrating and magnifying powers are very considerable, as will be shewn presently.

July 30, 1783, I viewed the nebula fourth preceding Flamsteed's 24 Aquarii, discovered by Mr. Maraldi, in 1746.

"In the small *sweeper*\*, this nebula appears like a telescopic comet."

Oct. 27, 1794. The same nebula with a 7-foot reflector.

"I can see that it is a cluster of stars, many of them being visible."

If we compare the penetrating power of the two instruments, we find that we have in the first  $\frac{\sqrt{.41 \times 42^2 - 12^2}}{2} = 12.84$ ; and in the latter  $\frac{\sqrt{.43 \times 63^2 - 12^2}}{2} = 20.25$ .

However, the magnifying power was partly concerned in this instance; for, in the *sweeper* it was not sufficient to separate the stars properly.

March 4, 1783. With a 7-foot reflector, I viewed the nebula near the 5th Serpentis, discovered by Mr. Messier, in 1764.

\* The small *sweeper* is a Newtonian reflector, of two feet focal length; and, with an aperture of 4.2 inches, has only a magnifying power of 24, and a field of view  $2^\circ 12'$ . Its distinctness is so perfect, that it will shew letters at a moderate distance, with a magnifying power of 2000; and its movements are so convenient, that the eye remains at rest while the instrument makes a sweep from the horizon to the zenith.

A large one of the same construction has an aperture of 9.2 inches, with a focal length of 5 feet 3 inches. It is also charged low enough for the eye to take in the whole optic pencil; and its penetrating power,

with a double eye glass, is  $\frac{\sqrt{.41 \times 92^2 - 21^2}}{2} = 28.57$ .

"It has several stars in it; they are however so small that I can but just perceive some, and suspect others."

May 31, 1783. The same nebula with a 10-foot reflector; penetrating power  $\frac{\sqrt{.43 \times 89^2 - 16^2}}{2} = 28,67.$

"With a magnifying power of 250, it is all resolved into stars; they are very close, and the appearance is beautiful. With 600, perfectly resolved. There is a considerable star not far from the middle; another not far from one side, but out of the cluster; another pretty bright one; and a great number of small ones."

Here we have a case where the penetrating power of 20 fell short, when 29 resolved the nebula completely. This object requires also great magnifying power to shew the stars of it well; but that power had before been tried, in the 7-foot, as far as 460, without success, and could only give an indication of its being composed of stars; whereas the lower magnifying power of 250, with a greater penetrating power, in the 10-foot instrument, resolved the whole nebula into stars.

May 3, 1783: I viewed the nebula between  $\eta$  and  $\varrho$  Ophiuchi, discovered by Mr. Messier, in 1764.

"With a 10-foot reflector, and a magnifying power of 250, I see several stars in it, and make no doubt a higher power, and more light, will resolve it all into stars. This seems to be a good nebula for the purpose of establishing the connection between nebulae and clusters of stars in general."

June 18, 1784. The same nebula viewed with a large Newtonian 20-foot reflector; penetrating power  $\frac{\sqrt{.43 \times 188^2 - 21^2}}{2} = 61,18$ ; and a magnifying power of 157.

"A very large and very bright cluster of excessively compressed stars. The stars are but just visible, and are of unequal magnitudes: the large stars are red; and the cluster is a miniature of that near Flamsteed's 42d Comæ Berenices. RA  $17^h 6' 32''$ ; PD  $108^\circ 18'$ ."

Here, a penetrating power of 29, with a magnifying power of 250, would barely shew a few stars; when, in the other instrument, a power 61 of the first sort, and only 157 of the latter, shewed them completely well.

July 4, 1783. I viewed the nebula between Flamsteed's 25 and 26 Sagittarii, discovered by Abraham Ihle, in 1665.

"With a small 20-foot Newtonian telescope, power 200, it is all resolved into stars, that are very small and close. There must be some hundreds of them. With 350, I see the stars very plainly; but the nebula is too low in this latitude for such a power."

*(To be concluded in our next.)*



## SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &amp;c.

*Sketch of Ornithorhynchus Paradoxus.* By M. BLUMENBACH. See our Journal, Vol. IV. page 506.

THIS animal, see Pl. XXIV. Fig. 4. was presented to the author by the Right Hon. Sir Joseph Banks. It abounds in a lake of New Holland, near Botany Bay. The form of its body, with the exception of the head, is nearly similar to that of a small otter. Its mouth is large, flat, covered with a naked skin, and is nearly similar to the beak of a duck. The edges of its lower jaw, are furnished, as in the abovementioned fowl, with small indentation, resembling the teeth of a saw. But notwithstanding the external resemblance, the head when dissected resembles that of other quadrupeds, but with this remarkable anomaly, that the two intermaxillary bones have an interval between them, which is filled only by cartilages. A great number of nerves, proceeding from the fifth pair, are distributed to the beak, and afford the animal all the sensibility necessary to seek its proper food at the bottom of the waters it inhabits.

This creature, from the form of its body, the shortness of its feet, and the membranes which unite its toes, is in some degree analogous to the *porcupine ant-eater*, *myrmecophaga aculeata* of Shaw, Natur. Miscell. No. 36: *Echidna* Cuv. tab. Zool. But it differs considerably from it in the form of its mouth, and the nature of its integuments. It is proper to observe, that the family of toothless animals, of which few species were known in the old world, is found to have several representatives in the vast continent of New Holland: thus also the class of didelphia may be said to exist in only few specimens in America and the Indies, compared to the various forms under which they are met with in this newly discovered country.

*Bulletin de Sciences, An. 8. No. 39.*

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*On a new Variety of Zircon, by CITIZEN HAUVY.*

The chrystals of Zircon which have hitherto been found in Ceylon, France, and elsewhere, have been transported by water into their different situations, and we had no indication of their native place, nor of the substances which served them either for support or matrix. The interesting travels which have lately been made by Citizen Lasterie into Sweden and Norway, have given us the knowledge of the primitive situations of this kind of mineral. Amongst the objects of natural history which he has collected, was a granite found at Fridichswern in Norway, and composed of red feld-spath and amphibolite with brown chrystals, known in that country by the name of Vesuvian, which name has been

given

given by the celebrated Werner to the substance which we call *Idocrasis*. Citizen Haüy has discovered that these crystals essentially differ in structure, as well as in their other characteristics, both from idocrasis and from brown tin grains, with which, at first sight, they might be confounded; and that they belong to the zircon, of which they present a new variety. Their fragments exposed to the flame of a candle, instantly lose their colour, and this also is the case with fragments of zircon. Their primitive shape, indicated by the direction of the natural junctures, is a rectangular octahedron, (Plate XXIV. Fig. 3,) having the same angles as those of the zircon, and like it, divisible by planes, which proceeding from the summits, coincide with the bases of the triangles, which form the faces of the octahedron.

The variety of which he treats, and which is represented by Fig. 3, has thirty-six faces.

Its sign is  $\begin{matrix} 1 & 2 \\ D & D^{\circ} & E^{\circ} & P \\ l & u & \kappa & P \end{matrix}$ . Citizen Haüy calls it the *subtractive zircon*, a denomination which he has adopted in cases, like the present, where one of the exponents which accompany the indicative letters, the letter E for instance, is less by an unity than the sum of the other exponents.

The following are the measures of the principal angles determined by the help of theoretical calculation. Incidence of  $l$  upon  $l$ ,  $90^{\circ}$ , of  $P$  upon  $l$ ,  $131^{\circ} 25'$  of  $\kappa$  upon  $P$ ,  $150^{\circ} 5'$ , and upon  $l$ ,  $142^{\circ} 55'$ , of  $u$  upon  $l$ ,  $159^{\circ} 17'$ , and upon  $P$   $152^{\circ} 8'$ .

The length of one of these crystals taken between the summits of the two pyramids is 18 millimetres, and the thickness 8 millimetres. Their colour is brown, mixed with orange; they are transparent, and they contain brilliant spangles, interspersed through their substance, which give the appearance of an aventurine.

*Bulletin des Sciences, No. 39, p. 16.*

*Extract of a Letter from CIT. HECTH, jun. Apothecary at Strasburg, to CIT. VAUQUELIN.*

I lately received a letter from M. Klaproth, in which he informs me, that having purchased at the shop of M. Hahneman, a certain quantity of the new kind of fixed alkali, which he pretends to have discovered, he found, by analysis, that it was nothing but refined borax.

Thus it is found, that this new fixed alkali, so pompously announced in the Journals by M. Hahneman, under the name of *pneum*, and sold by him for twenty francs the ounce, is reduced by the analysis of M. Klaproth to its proper value.



*W. John Smeaton's Improved Steam Engine on  
Savary's principle, with a separate Condenser &c. &c.*

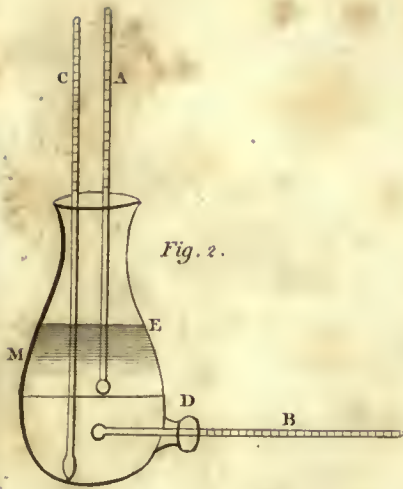
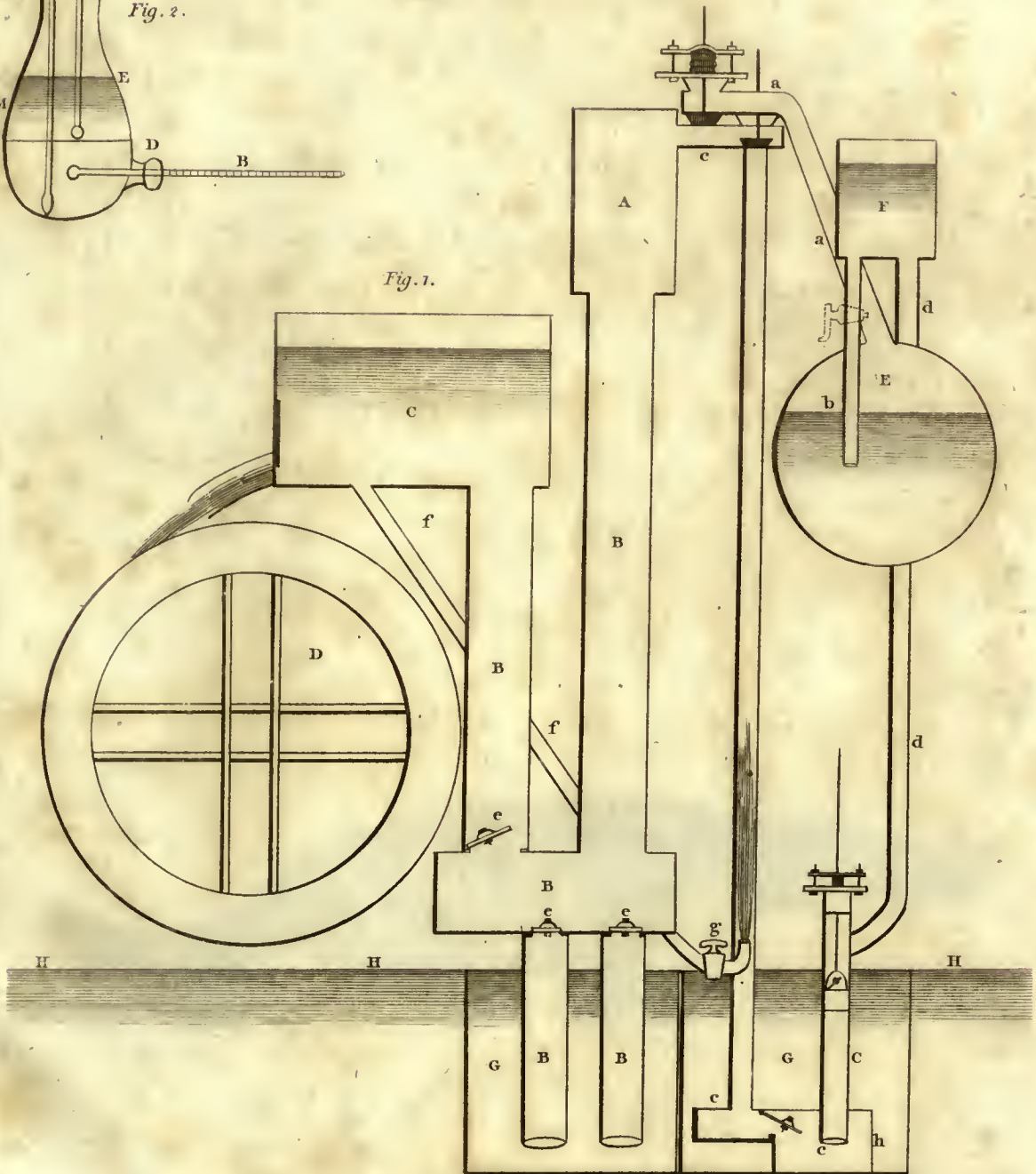
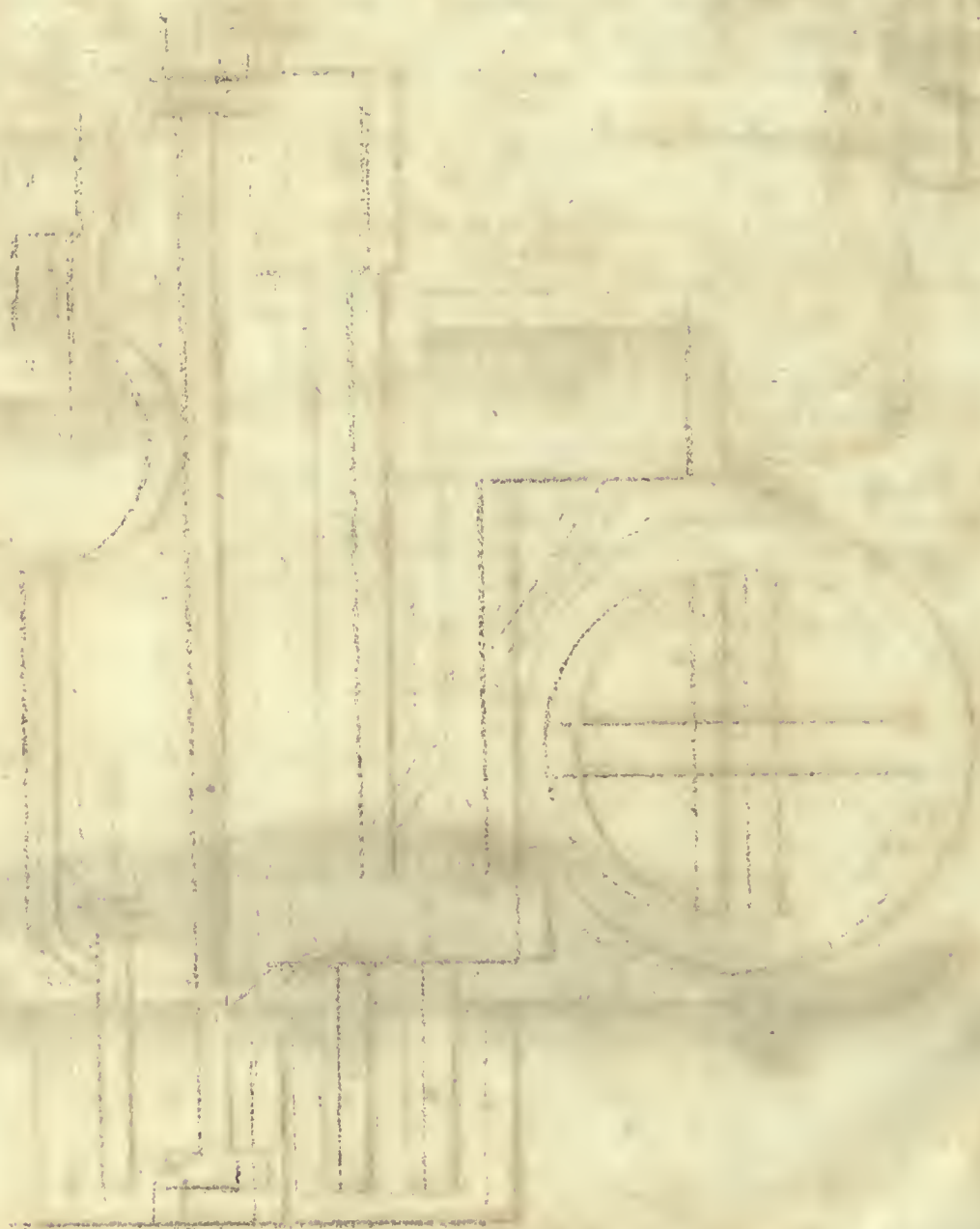
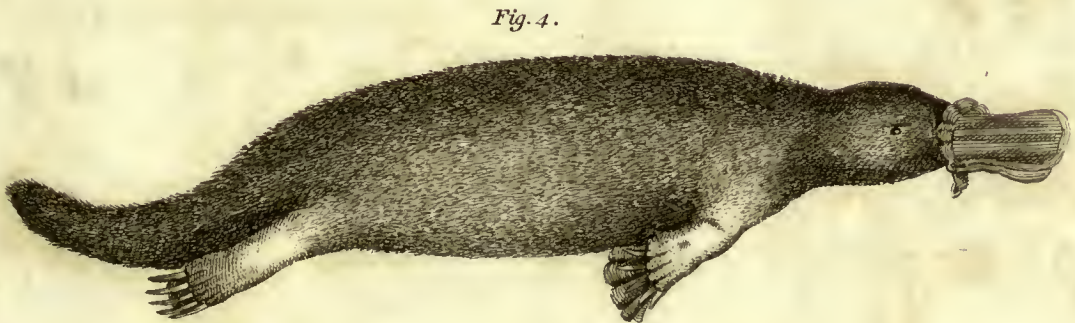
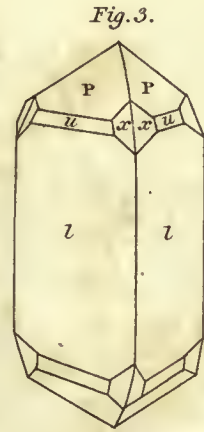
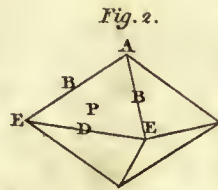
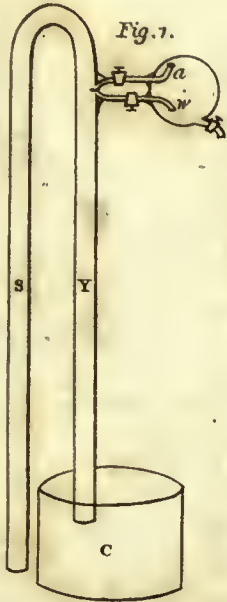


Fig. 1.













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